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Antiquity

VOL. XXVI No. 101 MARCH 1952

Editorial Notes

THESE Notes have more than once been written about primitive dwellings, but this is the first time they have been written *in* one. There is no better way of getting to understand any kind of house than to live in it for a time. This one made of mud, size 15 feet 6 inches by 13 feet 9 inches, height about 12 feet. The roof of twenty split palm-trunks, ten resting on each side of a single stout one supporting eir ends. These beams support a closely set matting of wattles, which in turn support e outermost layer of mud. The floor is of sand or hard ground, and the walls are white-ashed, about a foot thick and have four windows with wooden shutters. The room is rnished with wooden beds strung with local cord and two imported wooden armchairs. opens sideways into a covered porch leading to an open courtyard. There are other oms joined to this one by a wall and forming a square or rectangle.

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This room is a unit in the courtyard type of house of which there must be ousands in the Nile Valley. It has no special peculiarities; but it is interesting just ecause the type *is* so common. Simple as it is, far more thought has been given to it an the casual observer might suppose. Even the making of a mud wall 12 feet high will stand up and not crack is not a thing that can be done without experience ded down traditionally. The whole thing is an excellent example of adaptation to tions—here consisting chiefly of heat and cold, wind and sand. The walls tain the heat during the cold nights—and they can be very cold in mid-winter; and summer you can sleep outside in the court.

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The situation is at El Kab in the Sudan, at the top of the great bend where the Nile w south-westwards. Rain rarely falls here, and when it does it is a disaster, causing ne of the mud walls to disintegrate. This occurred in 1950 at Abu Hamed, 40 miles ther up; it would be interesting to know why some houses survived and whether it s due to any special structural features.

However, I did not come to El Kab to study mud architecture, but to make plan couple of forts which, though mentioned by Cailliaud and Jackson, were prac unknown. One of them proves to be quite a sophisticated affair, mainly of rough stone it is roughly rectangular, with solid roundish bastions at the corners and in the middle of each of the longer sides. The walls are never less than 9 feet thick and still stand to a height of 12 feet or more; the original height was much more because it was carried on in mud brick, traces of which survive in places. Inside the fort were round stone huts, some of whose interior walls were finished with a coat of plaster, painted red. Of the date it can only be said at present that it must be before A.D. 1500; later when the pottery (of which there is an immense quantity lying about) and the plans of the gateways have been studied, it may be possible to be more precise. Linant de Bellefonds made a sketch of the castles in 1822, and it proved very useful, as it shows features that have since fallen into ruin; it is reproduced in my *Fung Kingdom*, Plate 18.

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The excavation of such a site would be extremely difficult for all kinds of practical reasons, nor probably would the results justify the labour and expense. In this respect the stone fort of El Kab is typical of many sites in all parts of the world whose prime need is that they should be planned, photographed and described, and specimens of the pottery collected and placed in a museum for comparative study and for reference by future investigators. Making a plan is not technically difficult; at El Kab it took four days, but that was chiefly because the mere task of walking about and taking measurements over an irregular mass of fallen stone was lengthy and laborious (especially with only a single untrained Bishari as assistant). But there are many compensations, apart from the satisfaction of doing the job at all. There is no better way of getting to know a site than to plan it; one has to walk over and examine every portion, and one accumulates a really representative collection of sherds. One gets some idea of what the living conditions must have been like. And (here at any rate) one is rewarded by views of the majestic river, whose aspect changes from morning to evening but is always a delight to behold, and a never-ending wonder. Here it must be nearly a quarter of a mile wide; a little higher up it is a mile wide, and in one place nearly two miles, set with islands and bordered by green crops planted on the steep mud banks as the water gradually falls.

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There are many other sites to be similarly investigated between here and Atbara; only a few are marked on the map, and not one of them in the whole stretch of 200 miles has ever been examined by an archaeologist. It is an exciting prospect, and by the time that these Notes are being read I hope to have examined many of them. The map marks the ruins of a church at one place (Kuddik), and Jackson has seen another, with granite columns, now incorporated in a mosque (Artul Island). There are rock-pictures and castles of unknown age and character; and there is at least one early neolithic site (Gereif). There is also of course the probability of lighting unawares upon some important new site.

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Exactly one month has passed since I wrote the last sentence, and the proofs have reached me in another rather remote place. The forecast has been abundantly fulfilled Several more of the stone castles have been found and planned. Beside two of them

EDITORIAL NOTES

hurches built of red brick, the walls still six and ten feet high. Many of the pots y the castle-people had crosses incised just inside the rim. Christianity was the gion of the Sudan for at least 700 years ; it is usually supposed to have yielded place to slam soon after A.D. 1300, but the next two centuries are historically blank and it may ave lingered on longer in certain regions. In one place just outside a small Christian village were the flattened remains of tombs that were covered with a cement-like plaster and may have been precursors of the Mahometan *gubbas*. On some of these I found broken tiles with inscriptions in Greek letters. These, though fragmentary, should help to provide a much-needed date for the period of the castles. That is a job for experts.

⁂ ⁂

It is pleasant to think that two months still remain for more discoveries. Enough has been done already to show that there is a rich yield for field-archaeology in these parts, and that the principles thereof learnt in Britain are equally applicable here. They are in fact of world-wide validity. The art of interpreting superficial features demands, apart from practical experience, little more than keenness of observation, especially of minute and apparently trivial detail, combined with common-sense and imagination. Perhaps one might add patience and enthusiasm. It is desirable to work from a centre rather than to do a long trek; a few sites done thoroughly—photographed, planned and described—are better than many observed casually in passing. While travelling one can never spare enough time to examine a site thoroughly ; conversely, while planning it one gets to know it thoroughly, and one discovers new features almost every day.

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Some readers may be wondering whether work of this kind in the Sudan is affected by the political situation. It is not affected by it in any way at all, nor does it seem likely that it ever will be. Everywhere one meets with the greatest friendliness and hospitality, and if the transition to self-government is effected in an orderly manner, as one hopes, there seems to be no reason why future investigators of antiquities should not do their work as comfortably as the present writer is doing his.

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It is only proper that these Notes should conclude with a tribute of sincere thanks to the British Academy whose generous grant has made it possible for them to be written. Some account of the expedition is to be given to them in a lecture next November.

Sutton Hoo? a Summary

by THE EDITOR

MORE than a decade has passed since the ship-burial at Sutton Hoo was excavated, but owing to the war serious study of the finds has been of much shorter duration. Even so, in spite of much intensive research in laboratory and study, all of first-rate quality, certain vital problems still remain unsolved. We still do not know in whose honour the cenotaph was made; though we can say 'with complete certainty that no body, either cremated or inhumed, ever occupied the "body-space" at the west end of the burial-chamber, where everyone is agreed the primary burial should have been[1]'. Before the king can be identified the date must be determined, and that in turn may depend to some extent upon the coins. Since I first discussed this matter[2], further pronouncements have been made by coin experts[3], but there has still been no full and frank statement of the evidence upon which the date (A.D. 650–70) has been arrived at, and it is now long overdue. As a layman who has already somewhat rashly broken a lance with the coin experts, I feel that unless we are soon given something more substantial to discuss, the case for the date 650–70 may be said to have been lost by default. Dr Gordon Ward seems to think that that has already happened. The critical article of his which is printed below assumes a knowledge of Mr Bruce-Mitford's in the *Proceedings of the Suffolk Institute of Archaeology*[4]; for this reason, and because of its intrinsic importance, I have summarised it and printed it here. The summary has been submitted to Mr Bruce-Mitford and is approved by him as representing his own views correctly. Some of his arguments assume that the date of 650–70, handed out by the coin experts, must be provisionally accepted. But most of them would be just as cogent, and his main conclusion equally valid, if the date should eventually be put back about a quarter of a century and the cenotaph assigned to Redwald. O.G.S.C.

SUMMARY OF MR BRUCE-MITFORD'S ARTICLE

The purpose of his article was to clarify the broad position of the Sutton Hoo burial in English history and archaeology.

Some of the objects found resemble very intimately objects found in Sweden, thus indicating (for the first time) that there was a direct link with that country. This is agreed by all, but its meaning depends upon whether the burial was made for a stray Swede or for an established English king; and before even that can be discussed we must decide whether it was made for a king or not, whether it is Pagan or Christian, or whether it was a grave or a cenotaph.

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[1] Bruce-Mitford, 42, quoting references.

[2] ANTIQUITY XIV, 1940, 64–8. I see no reason to withdraw anything there.

[3] See *B.M. Provisional Guide*, 1947, 42: otherwise unpublished.

[4] Vol. 25, part I, 1959; sold separately (78 pages; price 5*s* 3*d* including postage) by the Society at the Ancient House, Buttermarket, Ipswich.

SUTTON HOO ? A SUMMARY

Is this a King's Burial ?

Yes. As Professor Chadwick pointed out kings were the repositories of all treasure and no one but a king could have possessed such wealth as was found. A *cenotaph* on this scale must surely have been for a king. The spoons and the ten (imported) silver bowls were probably royal gifts and, together with the other silver objects and those others (whose existence we may assume) which were not buried at all, are unlikely to have been accumulated except in a royal treasury and family. The theory that it is the cenotaph of a distinguished foreign visitor, though plausible, is unlikely to be correct, and seems disproved by the presence of certain objects (such as the whetstone and standard) which are claimed to be the prerogatives of the king. The whetstone is unused and certainly ceremonial, for it is unusable. 'It is a unique, savage thing, and inexplicable, except perhaps as a symbol, proper to the king himself, of the divinity and mystery which surrounded the smith and his tools in the northern world' (Kendrick). The iron standard was at first explained otherwise, but none of these other explanations can be sustained. Standards were the symbols of royal office peculiar to the Anglo-Saxon background, and ours has therefore an important bearing on the nationality, and so on the identity, of the person here commemorated. A standard was used by the Saxon Bretwalda (Overlord or High King) Edwin, who was also King of Deira (616–32) and had close relations with Redwald and the East Anglian court. Like the title Bretwalda the standard may well have a Roman lineage; and we know that the East Anglian dynasty of Wuffingas claimed a Roman Caesar (' Caser ') as one of their ancestors. Standards are shown on some of the earliest Anglo-Saxon coins, including a gold one of the 6th century which imitates one of the emperor Honorius. ' If the use of such a standard is thought of as part of a Bretwalda's ceremonial, then our object was no doubt made for Redwald, and passed on to his successors in the reduced days of East Anglian power, as a legacy from his reign '[5]

Is the Burial Pagan or Christian ?

Though grave-goods were buried in Christian graves and the custom was tolerated by Christian authorities, it was regarded as inefficacious rather than wrong and therefore tended to die out, the grave-goods assuming more and more a token character. The extravagance of Sutton Hoo is unlikely to have occurred in Christian times. The arguments of Professor Lindqvist, protagonist of the ' Christian ' case, are shown to ignore the historical setting and to be untenable. But by 650, ' the earliest date considered on numismatic opinion to be possible for the Sutton Hoo ship-burial, East Anglia was substantially converted ' ; so that the great burial must represent either a reversion to paganism, or the accession of a king who, even at this late date, still remained faithful to the old gods. Both alternatives are possible but open to objection. Any general reversion would surely have been mentioned by Bede, who records such elsewhere. If a pagan king has to be found, Aethelhere (d. 655), ally of the pagan Penda who had killed Aethelhere's most Christian predecessor Anna, is the most likely ; his death on a distant battlefield seems to fit admirably with the circumstance of a cenotaph.

But if it is the burial of an obstinate Pagan, how can we explain the ' positive and striking evidences of Christianity ' revealed by the objects found in the burial, such as the spoons with their patently Christian inscription and the crosses on the scabbard-bosses ? How can we explain the presence of such significant Christian objects, concentrated, intentionally as it seems, in the body-space of the burial-chamber, if the person concerned was in fact a pagan ? If we accept the date 650–70 we have to find some East Anglian king who satisfies these conditions. The only ones, it seems, are Aethelhere and Anna. There are some indications that Aethelhere may have been a pagan. But it is preferable to regard the Sutton Hoo ship-burial as ' a public and traditional monument erected in honour of a notable king [Anna] whose body had received Christian burial elsewhere '. A similar cenotaph occurs at Jellinge in Denmark, erected for a royal convert whose body was actually buried in a church; but in this case there were no grave-goods deposited. Anna died in 654, and is known to have been buried at Blythburgh. He was an outstanding king and the most likely one to be found within the given date-limits.

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[5] But if we accept the premise, is not the case for the cenotaph being Redwald's greatly strengthened ? O.G.S.C.

THE SWEDISH CONNECTION

Any interpretation of this aspect must be provisional, pending the definitive description of the major objects and the reconstruction and study of the less spectacular ones, which has not been carried out. These more modest objects may yet be archaeologically the most revealing. It must also await the excavation of the seven still intact barrows of the same group, and the publication of the finds from the three others.

Similarities between the Sutton Hoo material and Swedish finds of the Vendel period were noted as soon as the Sutton Hoo pieces were excavated, and appeared more striking when the shield and helmet were restored in 1946–7. But before drawing any conclusions from these similarities we must be sure that they are not merely the elements common to a widespread culture. For close parallels may also be found in the Rhineland and in Italy ; similarities in gold cloisonné work have been pointed out, and if such were the work of itinerant craftsmen, no significant connection with Sweden need be inferred from them. We find, however, that the similarities occur over a broad front and consistently, not merely in goldwork, so that we pass beyond the stage of isolated and dispersed parallels. (This is proved by a detailed analysis of several objects and of the art-motives depicted, which is hard to summarize and therefore omitted). It must also be noted that the two cultures also have a *custom* in common—that of boat-burial, which in the 7th century occurs nowhere else in Europe outside Suffolk and Uppland in Sweden. Thus the resemblances cannot be explained in terms of gifts or trade but betoken a more substantial link. Independent parallel development from a hypothetical common continental source may be ruled out. There was a direct and substantial connection between East Anglia and Sweden a hundred and fifty years and more before the first Viking raids. It seems to have been with Uppland, that is, with the kingdom of the Svear rather than with that of the Geats.

THE INTERPRETATION OF THE SWEDISH CONNECTION

The view that practically the whole of the Sutton Hoo treasure, including the jewellery, was imported from Sweden is rejected by all Swedish archaeologists and is untenable. For instance, chequered inlays of millefiori enamel do not occur in northern or continental metal-work, and are an insular phenomenon derived ultimately from a technique employed here in Roman times. It occurs in the escutcheons of the hanging bowls found in Anglo-Saxon graves. The use of millefiori at Sutton Hoo proves that the objects thus ornamented were made in England. Analysis also proves that all the other pieces (except the sword-pommel) are products, if not of the craftsmen who made the major pieces, at any rate of the same workshop or milieu.

The theory of a pagan Swedish king who invaded and conquered this region in or about 670 is ruled out by our knowledge of contemporary East Anglian history—which is much more extensive than Professor Nerman, author of the theory, seems to realise. Such a conquest runs counter to the whole tenor of Bede's narrative and could in any case hardly have been passed over without mention by Bede who was quite well informed about East Anglia at this period. (Professor Nerman's theory is discussed in detail and decisively rejected). We must therefore find some other explanation that fits the facts better. What we have to account for is ' the presence in an East-Anglian King's cenotaph of Swedish objects that were ancient when buried '. We have to explain the active Swedish influences at work as revealed in the regal East Anglian grave-goods ; and the following of Swedish usage for the funeral rites of an East Anglian king—if it be allowed, as it must be, that boat-burial may be a Swedish element.

' It seems to me that there could be one simple explanation, which would explain everything, and is positively suggested by the archaeological evidence as we have it at present. All the phenomena in the Sutton Hoo ship-burial that we have discussed are perfectly explicable if we suppose that Nerman's Swedish conqueror established his control over East Anglia not at the period of our burial—mid 7th century—but a good deal earlier ; in fact, that he was the man who founded the East Anglian dynasty in the mid 6th century. What we find in the burial would then be Swedish heirlooms (sword, helmet, shield) treasured as symbols of the origin or history of the royal house, and, in things that were made in East Anglia for Wuffinga patrons, traces of Swedish ideas and influences that would naturally spring from such roots. But by A.D. 650–70, the time of the Sutton Hoo burial, the dynasty having been established for a hundred years, or some four generations, would have become wholly absorbed in its Western European milieu and an integral part of Anglo-Saxon civilisation. Hence the perfect manner in which the burial as a whole fits into its insular setting. Once such a family link had been established between East Anglia and Sweden, we may suppose that contacts were from time to time renewed '.

Statements in the sagas quoted by Nerman that a Swedish king conquered a fifth part of England (described as Northumbria) would thus be seen to have some basis in historical fact, though attached to the wrong king and the wrong part of England. Since nothing is known of the origin of the Wuffinga dynasty there is no reason why we may not suppose that they came from Sweden about the middle of the 6th century. There are good reasons for thinking that, if so, they were an offshoot of the royal house of Uppsala, the Scylfings.

This explanation is substantially the same as that advanced by Professor Lindqvist, but was reached independently, and on broader grounds and a more detailed consideration of the evidence. Be it noted that one of the other barrows in the group at Sutton Hoo contained a boat. Some of the others may prove to have contained boats too. ' In the Swedish boat-grave fields, the boat-burials are only accorded to the heads of the family and they are accordingly strung out in time, about a generation apart. If the analogy with the Swedish boat-grave fields is to be pressed, the likelihood is that, if other boat-graves do exist, some of them will be appreciably older than the great ship-barrow. If boat-burial is a Swedish custom, its introduction into England would thus have taken place considerably before 650–70. This would imply that the contact with Sweden goes back into an earlier period '.

The repairs made to certain gold cloisonné buckles and mounts in the ship-burial show that they were of some age when buried. If these were English objects used to form a harness for a Swedish sword, we must recognise a Swedish cause operating an appreciable time before the Sutton Hoo burial.

The shield was old and dilapidated when buried, and is Swedish ; it would hardly have been brought over from Sweden in that condition and therefore probably fell into disrepair here. This again must represent Swedish contacts much earlier than the date of its burial.

The helmet, too, is Swedish and has been dated ' not much later than the Anastasius dish ' (491–518). This dating, though well grounded, must be regarded as ' no more than a strong body of opinion ' ; but the helmet certainly ' stands closer to late Roman prototypes than any of the Vendel or Valsgärde helmets, and . . . has also suffered some damage and been repaired ' ; so that it should be at least as old as the date suggested for the shield (*c.* 600 or earlier).

' In these ways the archaeological evidence conveys the impression not only of a pervasive Swedish influence at the highest social level, but of such influence extending back into the earlier days of East Anglia and its royal house. From what we know of the dynasty, much the most probable time for the introduction of such an influence would be at its inception '.

The Silver Spoons from Sutton Hoo

by GORDON WARD

THERE were found in the great ship burial two silver spoons, ten inches long, of a type well-known about the 6th century A.D. They have inscribed on them in Greek the names SAUL and PAUL (FIG. 1). Although several spoons of this character are known to students, their precise use remains obscure. Since they mostly bear the names of saints it is possible that they served some special liturgical purpose, but it is equally possible that they were strictly comparable to the christening mug of later ages (see discussion by Ernst Kitzinger in ANTIQUITY for March 1940). At Sutton Hoo the spoons were closely associated with a set of silver bowls bearing a cruciform decoration and were placed close to the right side of the position which should have been occupied by the head of the deceased. R. L. S. Bruce Mitford of the British Museum (*Proceedings of the Suffolk Institute of Archaeology*, Vol. xxv, 1949), writes as follows :—' We may accept the spoons without hesitation as a present for a convert—not a Christening gift for an infant, but as a gift intended to mark the baptism of an adult convert relinquishing his pagan state, and no doubt a royal convert '.

The long and important paper quoted above (and summarised by the Editor in the preceding article) is described by its author as embodying the official views about Sutton Hoo and thus claims very particular authority. It is in fact full of most valuable facts and helpful suggestions and will often be made use of in what follows. But its final decision that this is most probably the grave of King Anna or King Aethelhere seems to be without sufficient support. On the contrary, I believe that a careful consideration of these silver spoons and the associated silver bowls would lead us back to the view that this was Redwald's grave.

Bruce Mitford's reasons for regarding these spoons as baptismal gifts to an adult are that they obviously refer to a particular occasion in the life of St. Paul, namely, his vision on the road to Damascus, after which the orthodox Jew Saul became the Christian Paul. Since such spoons were usually inscribed with the names of the saints the inclusion of ' Saul ' (who was certainly not a Christian Saint) can scarcely be explained on any other hypothesis. We have therefore to look for a King of East Anglia who might have received so unusual a baptismal gift, and so valuable a present, as the silver spoons together with the adjacent set of silver bowls. Of the kings who might be in question :—

1. Redwald was baptised in Kent early in the 7th century.
2. Earpwald was converted by Edwin of Northumbria, and presumably baptised there about 627–8.
3. Sigberct was baptised in France sometime before he came to the throne, about 630–1.
4. Anna and his successors were no doubt baptised in East Anglia for Bishop Felix was there from about 635 to 654 and thereafter there was always a Christian centre in East Anglia.

One further point needs to be considered before we discuss which of these kings might first have owned the silver spoons. Bruce Mitford (loc. cit.) tells us that silver of this sort is excessively rare in Germanic graves, writing as follows :—'The fact remains that classical silverware, whether acquired by ecclesiastical gift or continental trade, is excessively rare not only in Anglo-Saxon but in Continental Germanic archaeology of this period. I can quote only the *phalerae* [a term covering various ornaments for men, women and horses. G.W.] from Ittenheim as instances of Mediterranean silver in the classical tradition occurring in a Germanic grave of this period (6th–7th centuries)'. This shows that Merovingian influence, which was reasonably strong in England during the 7th century, did not extend to the bringing in of this sort of silver which would not, in any case, commend itself to Merovingian ideas of true art. Indeed, when they looted silver in this form, the various barbarians who eventuated later in the royal races of France, etc., probably melted it down and converted it into torques or bracelets. We may deduce from this that the silver at Sutton Hoo was derived from the Mediterranean countries and perhaps from Rome itself. Bearing this in mind we may now consider further the list of baptisms given above. It is at once obvious that only Redwald is likely to have received a rich baptismal gift derived from the Mediterranean area. We know that he was baptised in Kent, possibly by St. Augustine himself, and we also know that the Pope sent rich gifts to the Kentish King. It may well be that some of these gifts were handed on to Redwald. Although he was then a young man his conversion was a matter of the greatest importance. The Augustinian mission had met with very little success outside the immediate sphere of Aethelbert's influence. It had failed with the Celts, and Sussex remained pagan for long afterwards. The Pope's intention that an Archbishop should be set up in London and another in York was utterly impossible of realisation. The conversion of East Anglia would at least have been a step towards a further advance and as such was certainly to be regarded as very greatly to be desired.

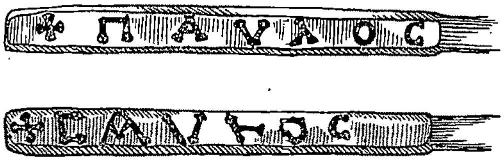

FIG. 1. INSCRIPTIONS ON THE SILVER SPOONS FROM SUTTON HOO

(Block kindly lent by the Suffolk Institute of Archaeology)

There was a further and even better reason for trying to enlist the goodwill of Redwald. His kingdom of East Anglia was increasing in power and began to threaten the predominance of Kent. Bede assures us that even in Aethelbert's lifetime Redwald began to contend with him for the Bretwaldaship. This may of course have been some time after his baptism but Aethelbert would be well aware of the threat long before it eventuated in specific action. In these circumstances the baptism of Redwald was a considerable political as well as religious triumph and is absolutely certain to have been marked by the presentation of exceptionally valuable Christening presents. We know that such gifts were available. It may therefore be deduced with considerable confidence that the silver spoons of Sutton Hoo (and perhaps also the silver bowls) were baptismal gifts to Redwald which he had carried back with him to East Anglia. This is advanced as a rational and not unsupported deduction explaining why this particular Germanic grave was found to contain silver articles of a sort almost unknown in others. The evidence of the silver spoons therefore points directly to Redwald, and there is still further evidence which points in the same direction, that is :—

1. The very unusual richness of the grave goods is agreed by all to suggest the burial of a great king and it is known that Redwald was far and away the greatest of East Anglian kings. He was the only Bretwalda in the family. He had not been afraid to befriend the exile Edwin when no other kingdom dared tolerate his presence, and he had later defeated the powerful king of Northumbria at the River Idle. He had before his death successfully contended with Kent. No other East Anglian king reached anything approaching the eminence of Redwald. It is true that we lack details of his predecessors Tyttla and Wuffa but the coin evidence excludes both of them, if not from great eminence at least from burial in this particular barrow at Sutton Hoo.
2. The large size of the mound, the largest in the group, and of the contained ship, are further evidence that here was buried a man of very great importance.
3. The fact that the burial was not in or near a church seems to rule out all but the pagan kings, Redwald, Ricbert the usurper and possibly Aethelhere, of whose religion we have no information. Redwald is the obvious choice from these three.
4. The presence of Christian objects in a pagan grave fits admirably the case of Redwald, who had both Christian and pagan altars in his place of worship to the end of his life. It fits no one else.
5. The other features of the burial are also such as we should expect if Redwald was buried here, e.g., the great Swedish shield so old that it had to be repaired before it could be used in the funeral ceremonies. They can only be harmonised with any other king by very specious reasoning.

Finally it may be noted that the iron standard provides a very suggestive link with Redwald. This requires more detailed discussion. We have practically no knowledge of the standards used by Anglo-Saxon monarchs except that Bede provides a very welcome sidelight on the grandeur of Edwin of Northumbria. He notes that Edwin had three different sorts of standard. In battle a banner was borne before him and when he went through the streets there was carried in front of him what the Romans called the 'tufa'. We are not concerned with these. The third type of standard seems to have been used on more ceremonial occasions, that is, when Edwin was riding with his Court 'between cities or villas or provinces'—which means when he was paying official visits. On such occasions, Bede says, 'semper antecedere signifer consuesset'. This means that a standard bearer used always to go before him, which may either imply that he rode in front of him or that he preceded him to the appointed place and there set up the standard. The sort of standard which was borne by a 'signifer' in Roman times is well-known. It had a metal point at one end for sticking into the ground and its upper part was elaborated with metal work and devices subsidiary to the main emblem at the top. In fact, such an iron standard as was found at Sutton Hoo falls readily into the group of standards borne by a signifier.

It is easily assumed that Edwin of Northumbria invented his own three standards after examining classical examples on coins, in manuscripts or conveyed to him by word of mouth. It may even be true that some of these sources were open to him, although this is rather difficult to believe since he commenced his reign as a pagan and, as far as we know, quite out of touch with Mediterranean civilisation. It is much more likely that he had seen in use, in East Anglia, this same iron standard from Sutton Hoo surmounted by its representation of a stag, for Edwin and Redwald were old friends. Edwin as an exile had spent a long time at Redwald's Court and must have been well aware of its usages. It was natural enough that he should introduce them in Northumbria when his years of good fortune came along. There may yet be found from Sutton Hoo the remains of a banner or tufa, to bear the standard company.

We can hardly be asked to suppose that the two kingdoms of East Anglia and Northumbria arrived quite independently at the idea that they ought to have standards carried before their kings. It is much more reasonable to believe that the idea spread through the known link between the two, the Edwin-Redwald link prior to 616. Are we then to believe that this is after all the grave or cenotaph of King Redwald ? The answer would be an unhesitating ' Yes ' if it were not for the alleged evidence of the coins. This has been allowed an importance which the facts in no way justify, and it is with regret that one reads Bruce-Mitford's statement that the year 650 is ' the earliest date considered on numismatic opinion to be possible for the Sutton Hoo ship burial '. As a matter of fact the numismatists have altogether insufficient dated material to enable them to give any confident opinion about Sutton Hoo, and they do not hesitate to say so.

There were forty coins in the purse. One of these seems to be that of a king Theodebert who reigned from 534 to 548. It is a forgery and might possibly be of later date. It should be remarked that forgery was very common about this time, probably because the official mints failed to meet popular needs. Three other coins, which were also imitations, bore the name of the Emperor Maurice Tiberius (582 to 602). These first four coins suggest a date for the burial quite early in the 7th century. The remaining 36 coins had no identification marks except the names of the towns at which they were struck and of the moniers who struck them. As it is reckoned that there were at least one thousand Merovingian mints in operation at this time these details are of no help to us. There were two coins in the hoard identical with some of the coins in another hoard which is thought to have been buried at Bordeaux not long after 670, and it is also said to be the fact that most of the 36 coins are of a sort which were current during the reign of Clovis II (A.D. 638 to 657). These two observations only serve to show that gold coins of this sort might remain in circulation for a number of years. They do not really assist when it comes to the question of dating the ship burial.

We will now consider what the experts have said about these coins. They were first of all examined by Mr Derek Allen of the British Museum who concluded that the Sutton Hoo burial ' cannot have taken place before about A.D. 600, is not likely to have taken place earlier than A.D. 640 to 650, and might even have taken place nearer A.D. 670 '. He would not, however, allow that the coin evidence definitely excluded the possibility that this was the grave of Redwald although he thought that it suggested a somewhat later date. Monsieur le Gentilhomme admits that the burial may be dated ' at the beginning of the second third of the 7th century ' which would be soon after 633. Redwald probably died somewhere about 625. Finally we have the opinion of Mr Dayrell Reed who states that ' the only evidence supplied by the bulk of the coins is that they might have been struck at any time between 615 and 675 '. He concludes that ' the jewellery, the silver and the historic facts all support a date of about 625 and only by straining the numismatic evidence can that be made to oppose it '. Mr O. G. S. Crawford, reviewing these opinions in ANTIQUITY for March 1940 reaches the conclusion that ' where such a range of possibilities exists, a difference of five or ten years cannot be detected ; and that is all that separates the numismatists from their archaeological and historical colleagues '.

It is evident that the numismatists have been set a task for which they are not equipped. They have no really accurately dated coin of the period under review. King Clovis reigned for 20 years and doubtless possessed coins which had been in the royal treasury long before that. A comparison between his treasure and that at Sutton Hoo helps not at all; yet this is the only sort of evidence which is available for anything like close dating. Moreover, the numismatists have very, very few examples which can be used for purposes of comparison. It is no fault of theirs if their conclusions are hesitant and not generally agreed or acceptable. In any case, it can be said with confidence that none of those quoted would dare to assert that it was impossible that the purse at Sutton Hoo can have belonged to king Redwald.

The final conclusion is therefore that same conclusion to which Professor H. Munroe Chadwick came in 1940, which he expressed as follows:—'If the burial dates from 600 to 640, all probability is in favour of the great and wealthy high King Redwald, who seems to have died about 624–5. He may possibly have buried the objects himself at the funeral of a relative; but what we know of the personnel of the period suggests that his wife was the person responsible, and consequently that the person commemorated was probably Redwald himself'.

No single numismatist has come forward to say that the burial cannot date between 600–640 and it is still for those who say that this cannot be the grave of Redwald to prove their thesis.

The Continental Home of the English

by H. JANKUHN

(Kiel)

[In April, 1938, I had a short holiday in Schleswig, chiefly in order to see and walk along the Danewerk. During my stay there, Professor Jankuhn took me to see the province of Angeln; and on a little hill above the small town of Süderbrarup we inspected the much disturbed remains of a circular stone-setting, perhaps once covered by an earthen burial-mound (barrow). The spot is marked B on the plan (FIG. 7), and is to be seen also on PLATE I. It was for centuries the meeting-place of the district, at which discussions were held, just as at our early Hundred Courts, to which it may have corresponded. Here, said Dr Jankuhn, the Angles of Angeln must surely have met to discuss their forthcoming expeditions to England. It is seldom possible to nail down decisive historical events to a particular spot; and this revelation seemed to me most dramatic and would, I said, make an admirable article for ANTIQUITY. But other events supervened, and this earlier invasion receded into the background. Now at last the article has come into being, not quite perhaps in the form originally planned, but none the less welcome. O.G.S.C].

ARCHAEOLOGISTS now regard it as certain that the Saxons who took part in the settlement of the British Isles in the 5th and 6th centuries A.D. came from the region between the Elbe and the Weser; but the continental home of the Angles is still, rather strangely, a subject of controversy.

Bede states unambiguously that the Angles came from the region called Angulus, which lies between the Schlei and Flensburg[1] and still bears the name of Angeln. Ptolemy put the Angles much further south on the middle Elbe, next to the Langobards.[2] Tacitus, in his description of the peoples of Central Europe mentions the Angles as members of a cultural community united in the worship of the goddess Nerthus; but he gives no clue to their area of settlement.[3] On the basis of this evidence, and some minor written sources, three theories have been put forward about the origin of the Angles. The theory that the name Angli was developed autochthonously in England needs no discussion.

Besides the historical sources we also have, as aids in the solution of the problem, the Sagas, the development of the language, place-names and archaeological finds.

The examination of the Sagas starts with the Offa tradition[4]—that this Saga reflects facts of Anglian history on the continent, and that it came to Britain with the immigrating Angles[5]. The geographical context there is given by the proximity of the Danes. The

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[1] Bede I, 15.

[2] Th. Steche, *Altgermanien im Erdkundebuch des Claudius Ptolemäus.* Leipzig 1937.

[3] Tacitus, Germ. cap. 40.

[4] G. Schütte, *Gotthiod und Utgard.* Kopenhagen 1935 and 1936. Herm. Schneider, *Germanische Heldensage,* two volumes, Berlin 1928, 1933 and 1934. Siegfried Gutenbrunner, *Schleswig-Holsteins älteste Literatur.* Kiel 1949.

[5] Jnger N. Boberg, 'Die Sage von Wermund und Uffe'. *Acta phil. Scandinavica* 16, 1942, p. 129 sequ. and G. Schütte, ibid, p. 233 ff.

Saga of Offa is, however, unknown amongst the German heroic Sagas, so that it could not have originated in, say, Thuringia on the middle Elbe. All the clues to the localisation of the Offa Saga point to Schleswig as the place of origin of the oldest Offa Saga, and so indicate Schleswig as the home of the Angles, whose king was Offa.

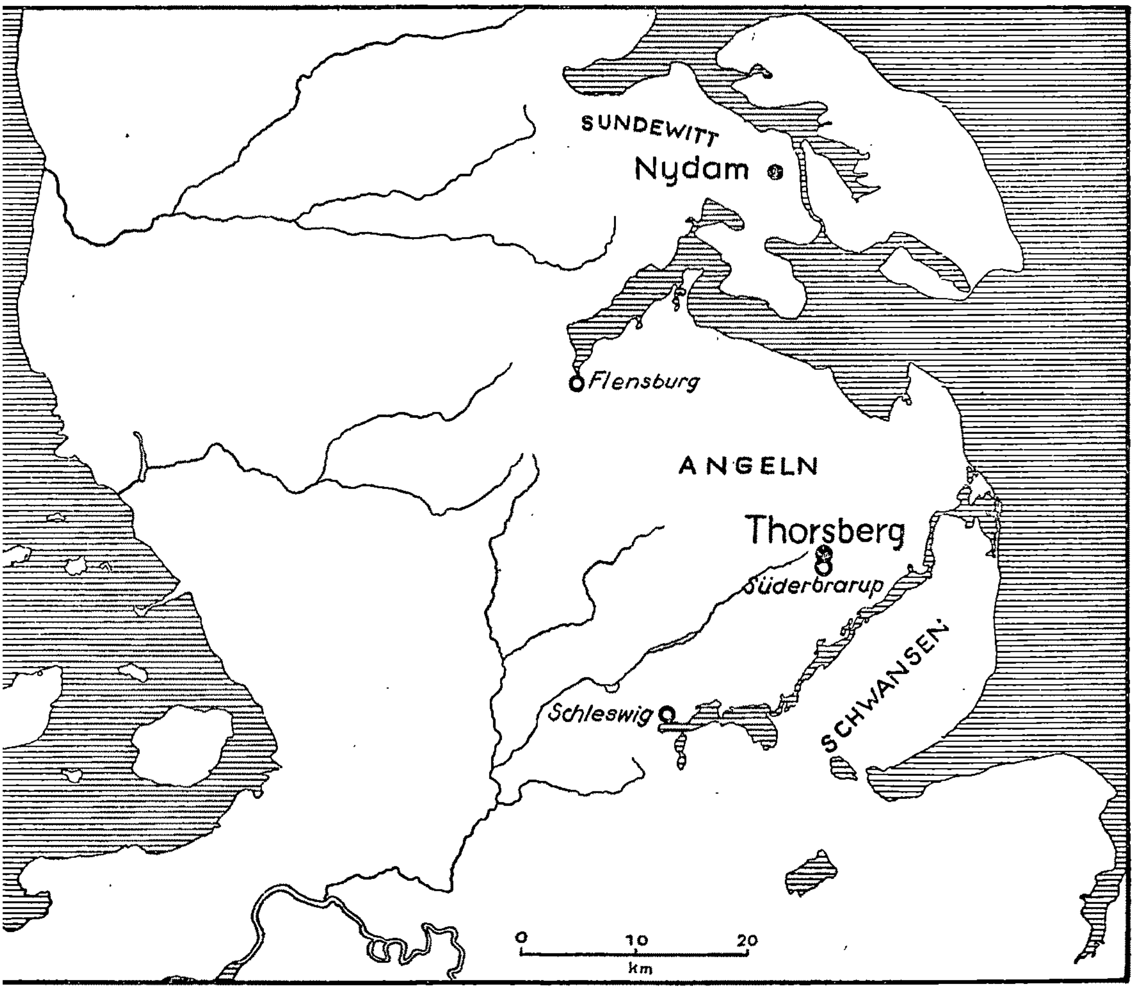

Fig. 1. CENTRAL SCHLESWIG SHOWING POSITION OF NYDAM AND THORSBERG

Philologists consider that Old and Middle English traditions and modern English dialects point to Schleswig or Jutland, lying to the north of the old Saxons, as the continental home of the Angles[6]. There is no evidence from the place-names of A.D. 500 and onwards of any special connection between Thuringia and the Anglian settlements

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[6] R. Jordan, *Eigentümlichkeiten des anglischen Wortschatzes*. Heidelberg 1906. E. Schwarz, 'Das angelsächsische Landnahmeproblem. German–Roman'. *Monatsschrift*, 32, 1950, p. 44.

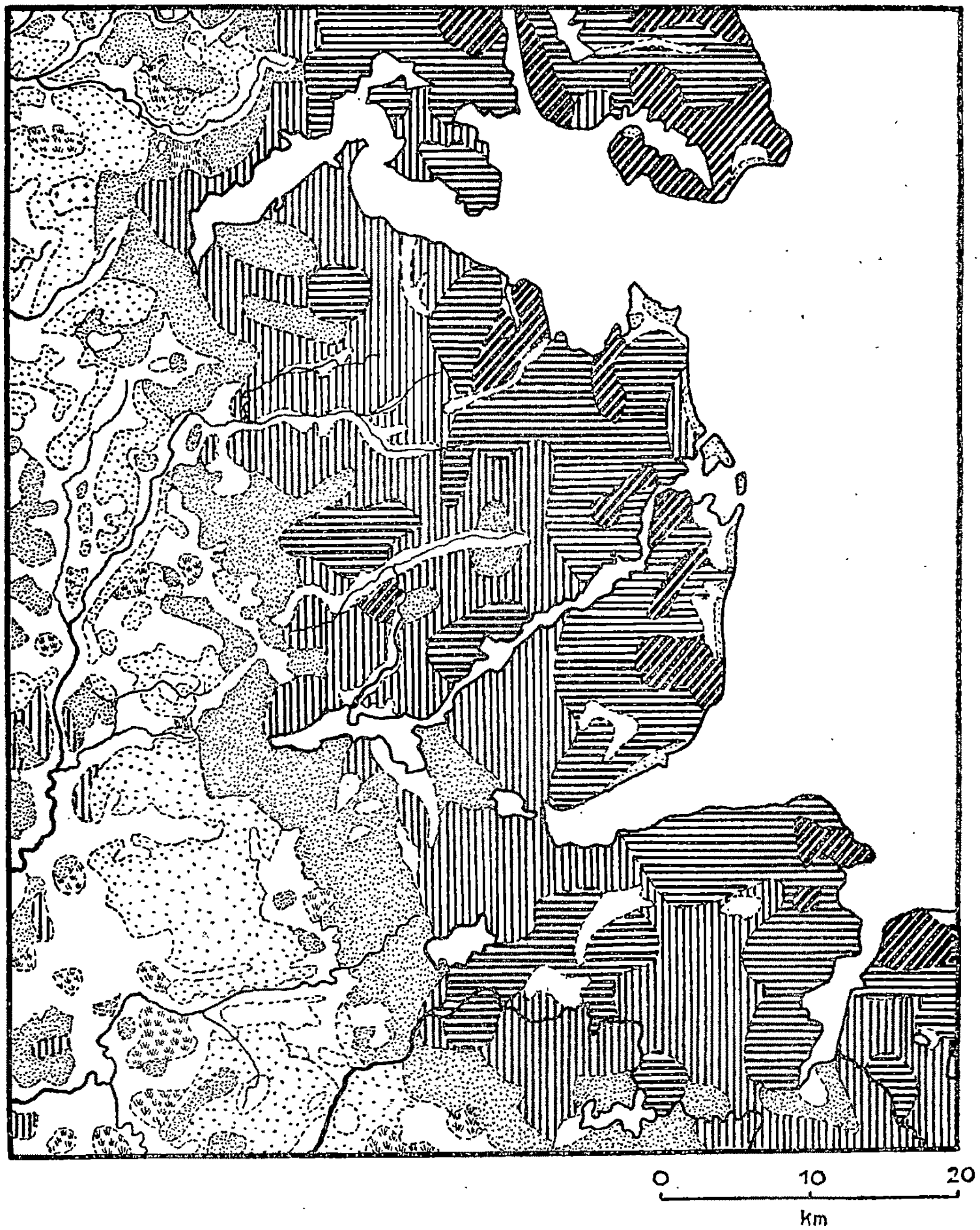

FIG. 2. SOIL MAP
Horizontal and oblique lines=heavy clay. Vertical lines=sandy clay. Dots=clayey sand

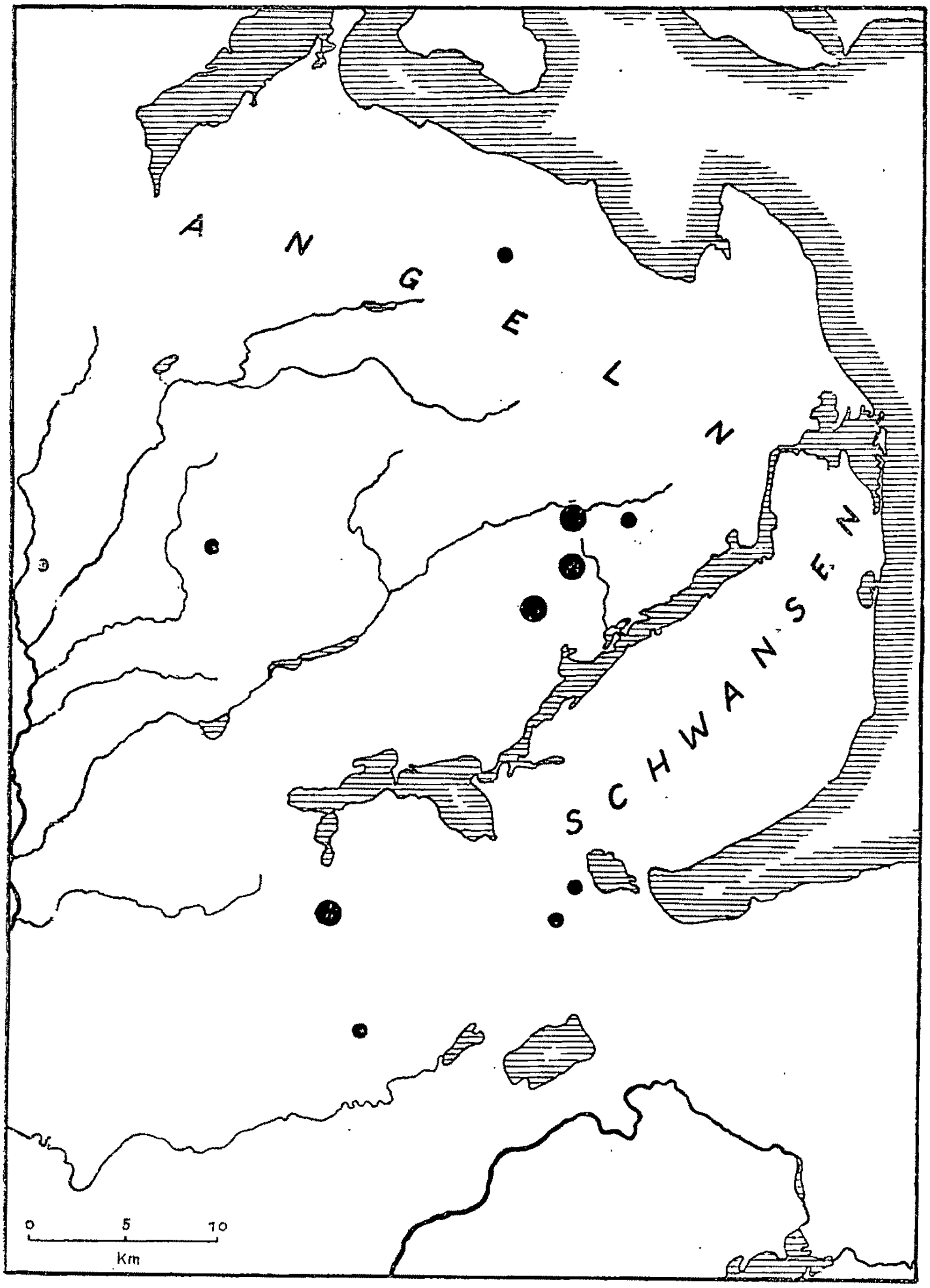

Fig. 3. CEMETERIES OF THE PRE-ROMAN IRON AGE (2nd AND 1st CENTS. B.C.)

in England. But a few isolated place-names in Schleswig, such as Lipping in Angeln, do recall similar names, such as Lipping, Leicestershire, in England[7].

So far as the archaeological evidence goes, a recent examination of the finds from the middle Elbe region during the first six centuries of the Christian era has shown no evidence of any connection with England during the 5th and 6th centuries[8]. Moreover, enough is known of the history of this Elbe region, where Ptolemy put the Angles, to show that it was almost uninhabited during the 1st and 2nd centuries A.D.[9] In any case there is no evidence that there was any large emigration from here across the North Sea during the 5th century.

The history of the settlements in the Angeln region has been studied intensively during the last 20 years[10]. It is found that, during the Bronze Age, the regions of Angeln and Schwansen were densely populated, the population appearing to become less dense towards the end of the period, as shown by the increasing scarcity of finds. There are no burials of the Early Iron Age recorded for either region, though plentiful in adjacent regions to the north and south. In the 2nd century B.C. cemeteries appear again, occurring not on the (mostly heavy) clay soil but restricted to sandy patches within the heavy clay area. (FIGS. 2 and 3). The finds from these urn-field cemeteries are identical, especially in regard to the pottery, with those from the region between Hamburg Lüneburg and Schwerin[11]. Other typical implements and ornaments, such as the 'Holsteinische gürtel', point to southern Holstein as the place of origin of these new inhabitants of Angeln[12]. Furthermore, there appear, though in smaller numbers, urns[13] and ornaments of a type common in Jutland, and in Southern Sweden. If this were not merely due to trade, it must mean that some of the new colonists came from the north. There are also some special native forms which developed in Angeln during the 1st century B.C.

The archaeological evidence suggests that the numerous Bronze Age folk of Angeln emigrated, possibly driven out by a worsening climate, and that both Angeln and Schwansen remained uninhabited for several centuries. To test whether these regions really were abandoned at the end of the Bronze Age, a method was used whereby the history of corn-growing can be determined by means of grain-pollen preserved in peat-bogs. Since agriculture had already been established for many centuries in Central and Northern Europe, periods of dense population should be revealed by the pollen-graph by a great abundance of grain-pollen, and periods of depopulation by its absence. Palaeobotanic research carried out in two bogs confirmed this inference, showing that agriculture had almost ceased at the end of the Bronze Age[14]. The absence of burial-finds corresponds nicely with a cessation of agriculture.

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[7] Kristian Hald, *Stednavne i Angel. Knudsen-Kretzschmer, Sydslesvig* II. Angel, Kopenhagen 1945, p. 70 ff. W. Laur, 'Angelns älteste Ortsnamen'. *Jahrbuch des Angler Heimatvereins* 14, 1950, pp. 133–40.

[8] H. Jankuhn, 'Zur Frage nach der Urheimat der Angeln', *Ztschr. d. Ges. für Schlesw.-Holst. Gesch.*, vol. 70–1, 1943, p. 20 ff.

[9] Fr. Kuchenbuch, 'Altmärkische Funde des 1 und 2'. *Jahrhunderts n. Chr. Sächsisch-Thuringische Jahresschrift* 1936, p. 211 ff.

[10] H. Jankuhn, 'Siedlungs- und Kulturgeschichte der Angeln vor ihrer Auswanderung nach England'. *Jahrbuch des Angler Heimatvereins* 14, 1950, pp. 54–132.

[11] ibid, plate 1, a, b.

[12] ibid, plate 1, c.

[13] ibid, plate 1, e.

[14] By Dr Schüttrumpf, Kiel.

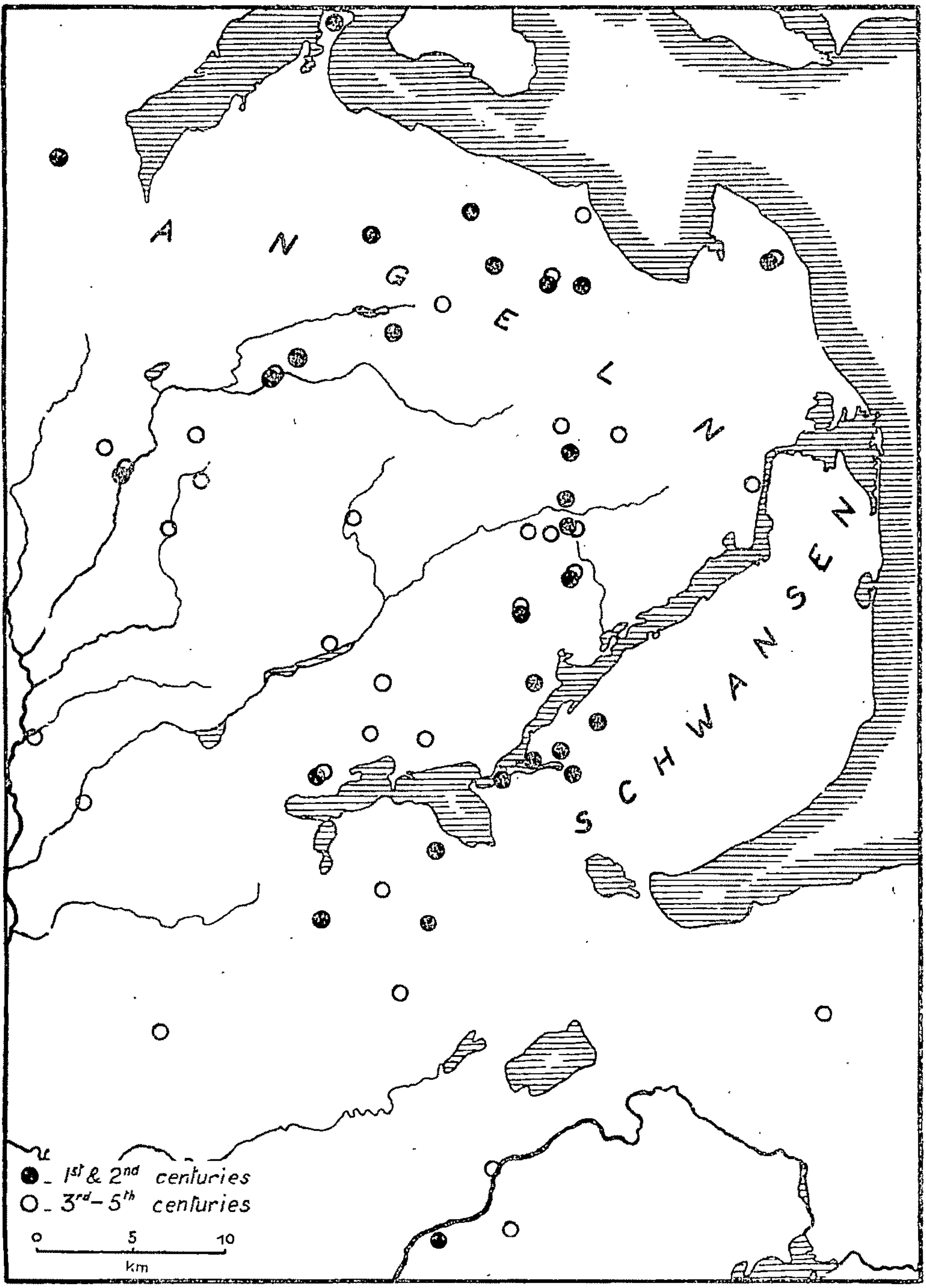


Fig. 4. CEMETERIES OF THE ROMAN PERIOD

But it seemed desirable to carry out some kind of test, to find out whether this cessation of agriculture might not have been due to some change in farming economy caused by a deterioration in the climate rather than to an abandonment of the Settlements.

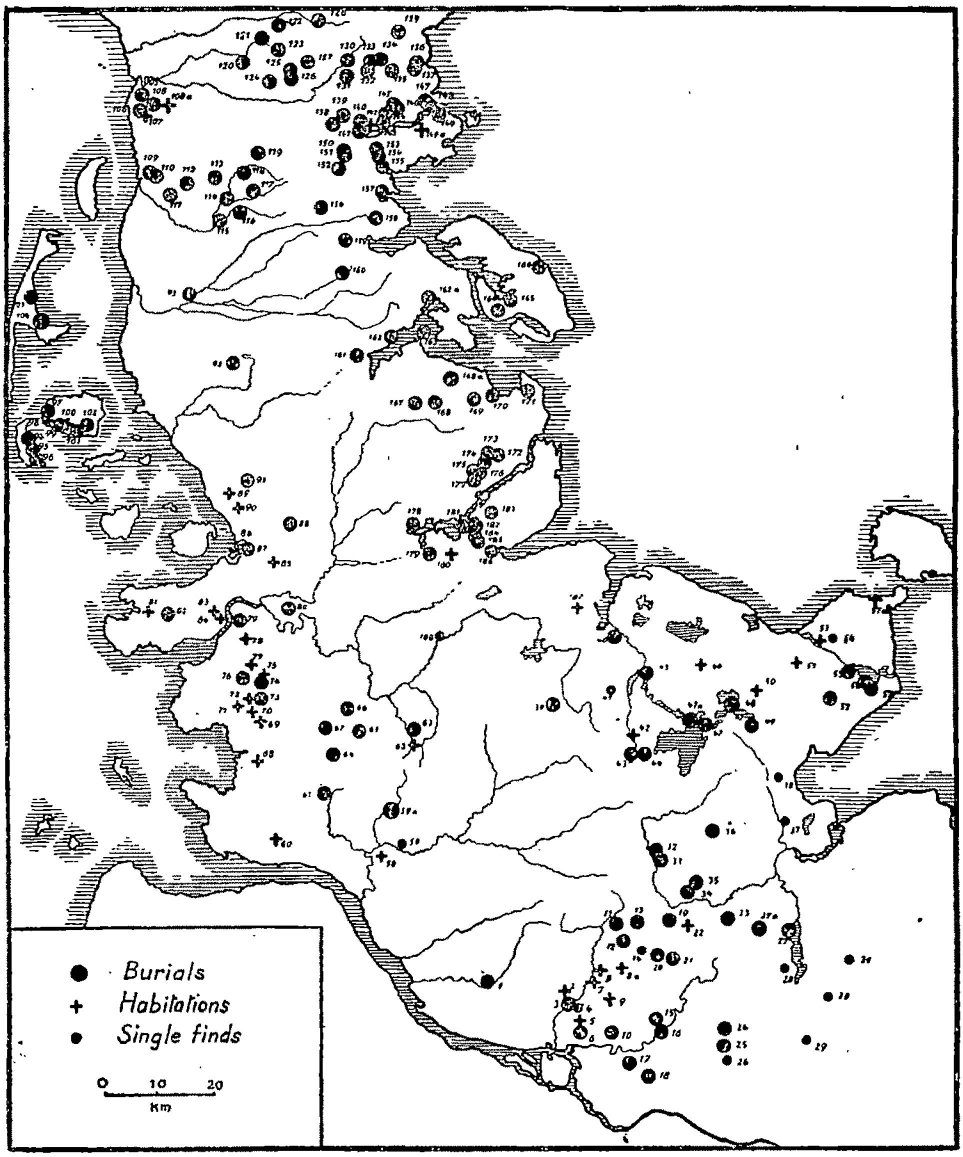
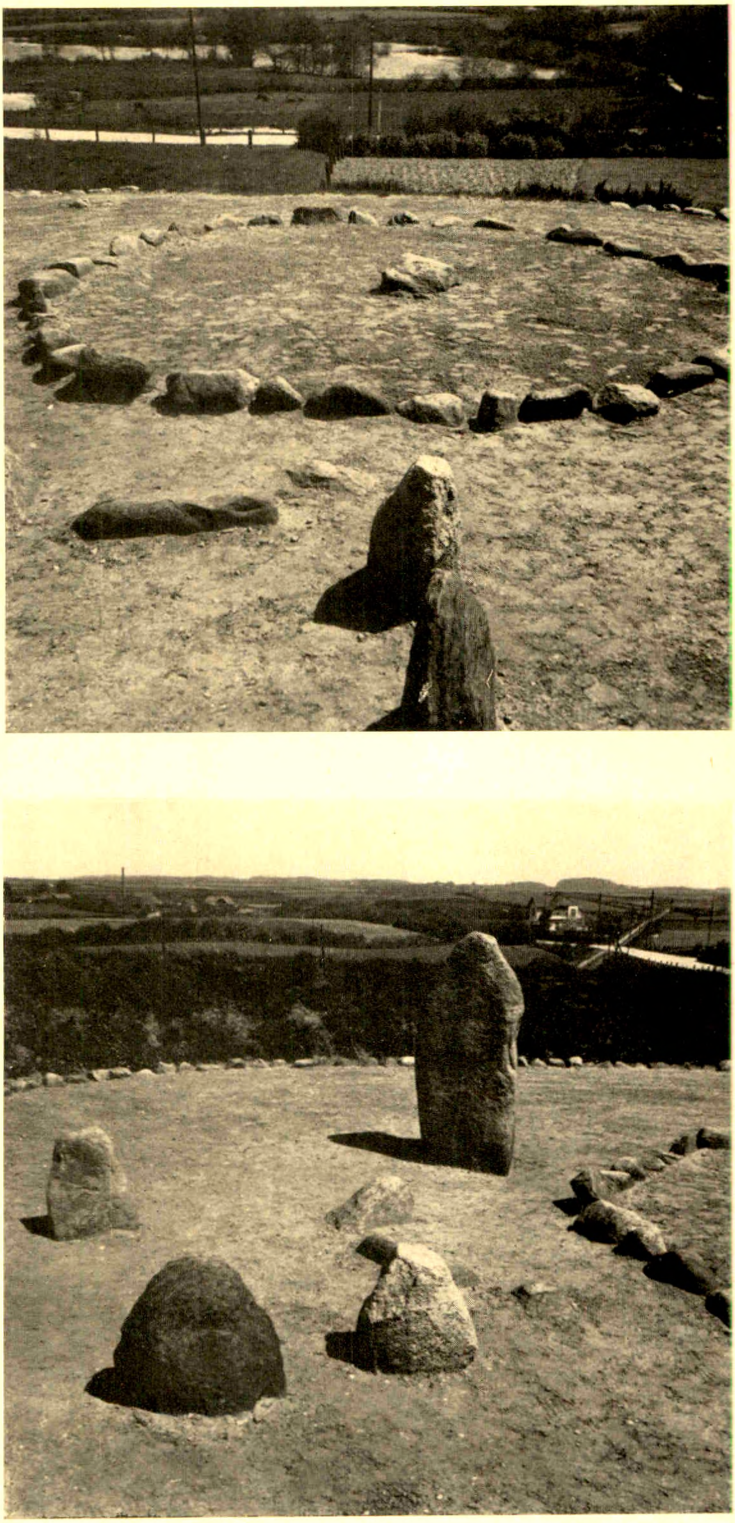


FIG. 5. SITES OF 1ST AND 2ND CENTS. IN SCHLESWIG-HOLSTEIN (*after* Tischler)

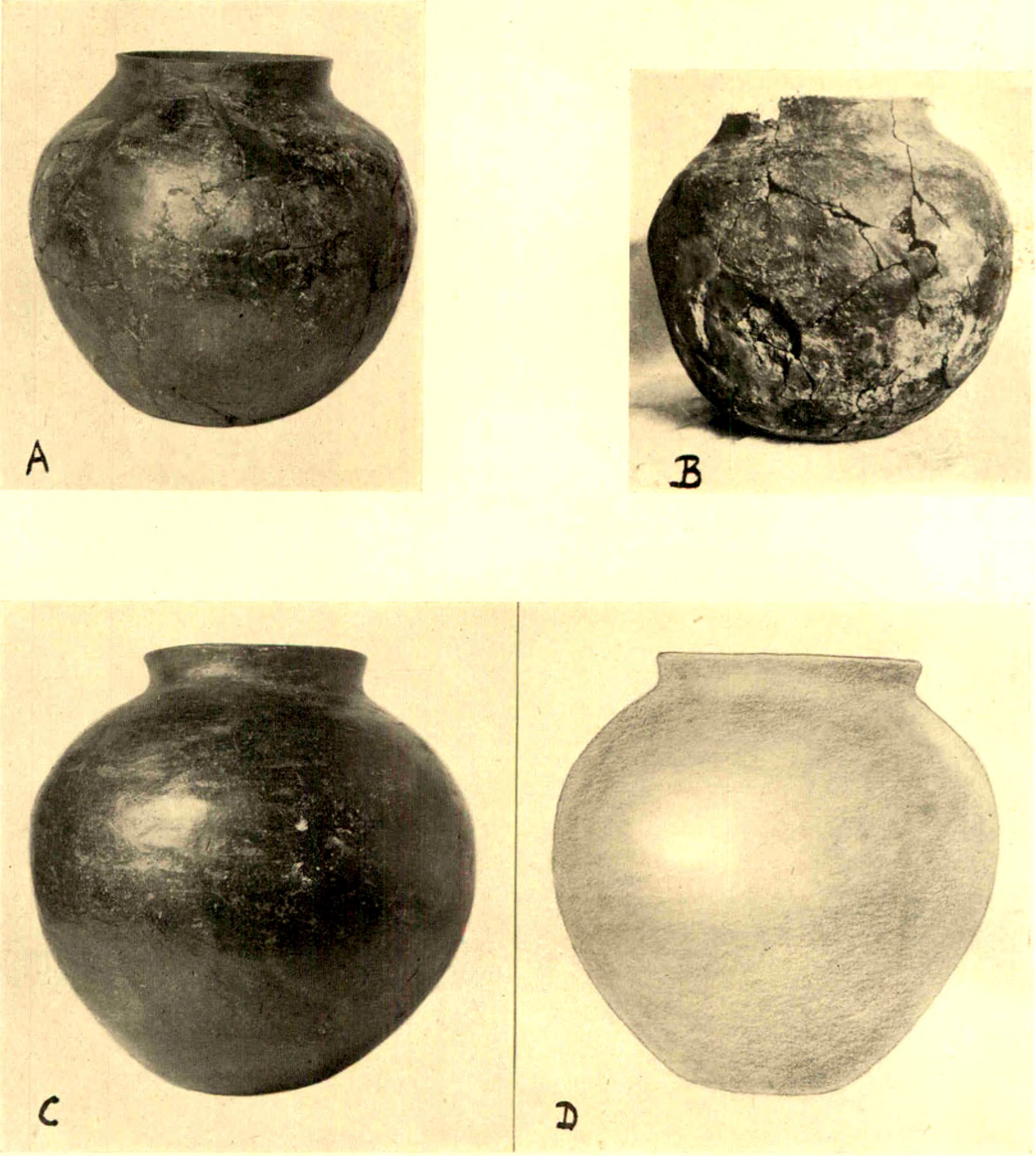
With this object in view *Plantago* pollen was examined, since recent Danish work has shown that it is a good indicator of the density of human habitations. Pollen from the Thorsberg bog show *Plantago* pollen becoming scarce at the end of the Bronze Age, just like that of grain.

PLATE I



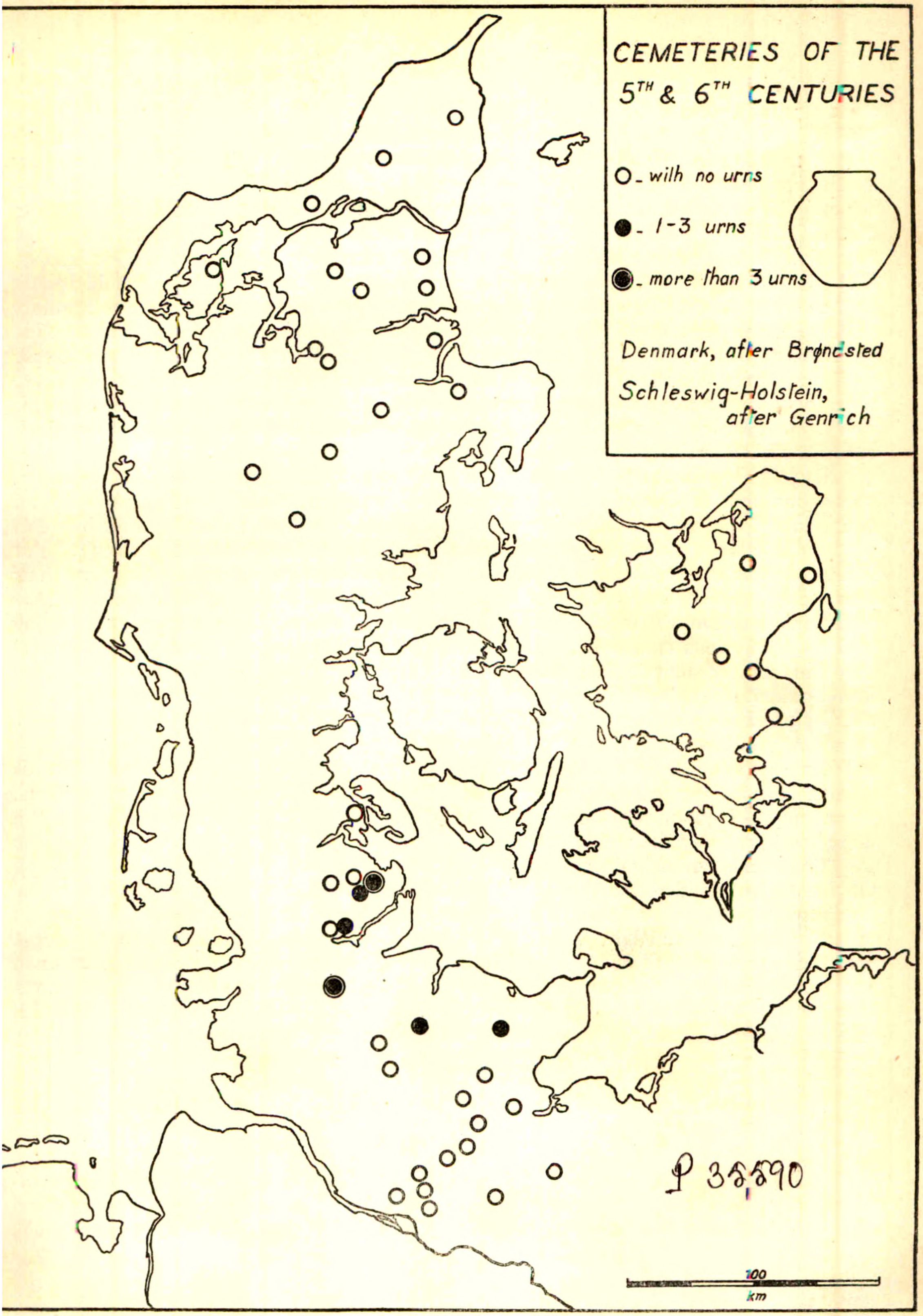
STONE CIRCLE AND STANDING STONE ON SITE OF FORMER BARROW; PERHAPS THE MEETING-PLACE OF ANGELN (B on the map, fig. 7)

PLATE II



CONTINENTAL (left) AND ENGLISH (right) URNS COMPARED

A, C from SÜDERBRARUP (1 : 4). B from SUTTON COURTENAY, BERKS. D from LACKFORD, SUFFOLK


CEMETERIES OF THE 5TH & 6TH CENTURIES
○ - with no urns
● - 1-3 urns
◉ - more than 3 urns
Denmark, after Brøndsted
Schleswig-Holstein, after Genrich
100
km

P 35590

FIG. 6

Archaeology and palaeobotany thus both show that the dense population of Angeln and Schwansen ceased at the beginning of the Iron Age. What happened to the Bronze Age folk is not known; presumably they emigrated elsewhere. Agriculture ceased almost entirely, and the deserted countryside went back to forest. In the 2nd century B.C. a new period of settlement began, the people coming mostly from the lower Elbe region. They settled first on the light sandy soils (FIGS. 2 and 3). About the beginning of the Christian Era a period of internal colonisation began, connected probably with extensive forest clearance (FIG. 4). On a map showing population-density in Schleswig-Holstein during the first centuries of the Christian era (FIG. 5). Angeln would appear as the dense nucleus of a settlement-area extending north and south beyond the frontiers of Angeln, and separated from adjacent regions on the north, south and west by a wide uninhabited belt[15]. With the increase in the population went a simultaneous increase in agriculture. On the western frontier of Angeln plentiful deposits of bog-iron ore were found and a large smelting centre grew up. It is reasonable to suppose that this well defined settlement area in south Schleswig, separated from the west by waste land, constituted the territory of a tribe.

During the years 1858–61 a great find was made in the centre of the oldest Iron Age settlement area in Süderbrarup, consisting of weapons, tools, pottery, textiles and ornaments[16]. It was considered to be of a sacrificial character, dedicated after battle as part of the booty; thus these big bog-finds would be evidence of tribal battles[17]. But in 1935 it was shown that the objects in the Thorsberg find ranged in date over four centuries[18], so that they could not have been deposited on a single occasion, but at intervals, as votive offerings. The period covered lasted up to about A.D. 400, during which the offerings changed[19]. First came pots (as elsewhere in Europe), which had contained butter, animal fats and bones, hazelnuts, flax. With the pots have been found animal skins, horns of cattle and querns. Sometimes there were heaps of stones near by on the bog and (rarely) wooden human figures. The pots are the offerings of a community of farmers. At about A.D. 100 a change occurred and the pots were superseded by metal ornaments belonging to clothes and weapons. From A.D. 200 onwards Roman imports appear, and in the 3rd century isolated gold objects, mostly armlets. From the large quantity of offerings it is legitimate to infer that Thorsberg 'served' a large area, probably the whole of Angeln.

The immediate neighbourhood of Süderbrarup abounds in Iron Age sites (FIG. 7). On the high ground round the Thorsberg bog are about twenty barrows, some of the Late Bronze Age, with secondary interments of the Early Iron Age and Roman periods; others were constructed in Viking times. In the middle of the area are two large cemeteries, one near the church and in the market-place having contained more than 1000 urn-burials dating from early Roman times down to the 6th century; 1300 graves have already been found. The other, north of the railway station, consisted of between one and two thousand burials, but nearly all of them were destroyed, only about twenty urns (3rd to 6th century) surviving. East of the sacrificial bog is a holy well to which, according to an old tradition, processions were led in pre-Christian times. Near it have been found several small pits, perhaps hearths, dating from the 1st century A.D. All this is evidence of the outstanding importance of the area in early times. It is to be inferred that Thorsberg was the religious, and probably also the political and economic, centre of the region.

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[15] F. Tischler, *Fuhlsbüttel, ein Beitrag zur Sachsenfrage*. Neumünster 1937, map I.

[16] C. Engelhardt, *Thorsbjerg Mosefund*. Kopenhagen 1863. H. Jankuhn, *Nydam und Thorsberg*. Neumünster 1950.

[17] J. J. A. Worsaae, *Om Slesvigs eller Sønderjyllands Oldtidsminder*. Kopenhagen 1865, p. 52 ff.

[18] H. Jankuhn, 'Zur Deutung des Moorfundes von Thorsberg'. *Forschungen und Fortschritte* 12, 1936, p. 202.

[19] H. Jankuhn, 'Die religionsgeschichtliche Bedeutung des Thorsberger Fundes'. ibid, p. 365 ff.

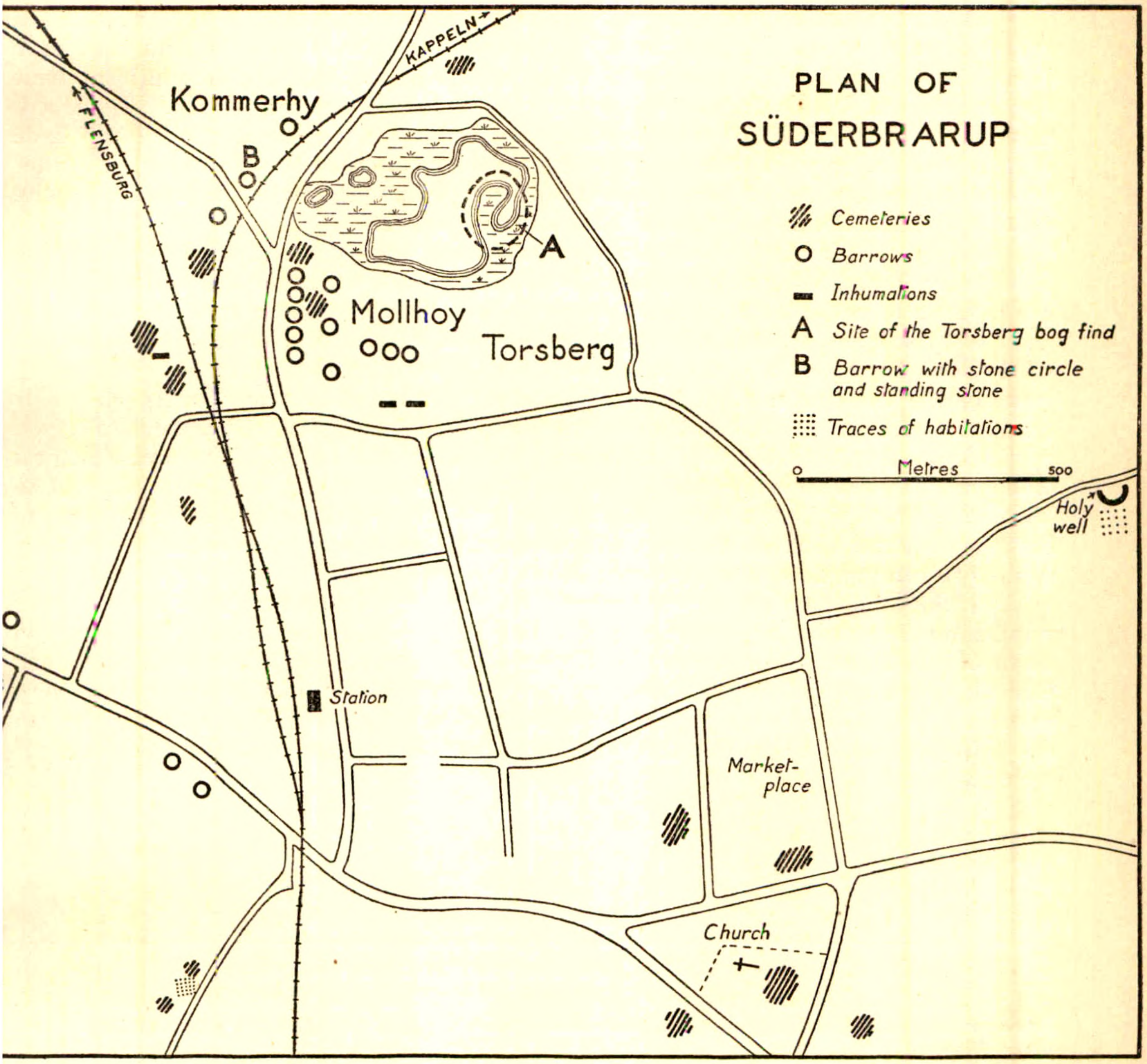

FIG. 7

In the years 1859–61 an uncommon type of sanctuary was discovered at Nydam in the middle of which were two or three large boats[20]. Only one of these—the one made of oak—was preserved[21]. In and around the boats had been deposited coins, iron weapons and (more rarely) ornaments. The finds cover the period 2nd–3rd to 6th century A.D. Nydam, like Thorsberg, is a sacrificial site, used over a long period, but the finds indicate another kind of rite; in the middle were the boats, and the offerings were mainly weapons.

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[20] C. Engelhardt, *Nydam Mosefund.* Kopenhagen 1865.

The settlement of southeastern Schleswig reached its climax in the 3rd and 4th centuries A.D. (FIG. 4). Judging from the number of the cemeteries the land must have been densely populated. From the fact that the belt of waste land in the south and west was encroached upon one may suppose that good soil was getting scarce. In the 5th century the cemeteries decrease in number and in the 6th they cease altogether. Bede says about A.D. 730 that the country of Angulus was deserted. He puts it ' inter provincias Jutarum et Saxonum ', a position corresponding to that of the present Angeln. Not until about A.D. 800 do signs of re-occupation appear.

During the 4th and 5th centuries various types of pottery were made in southeastern Schleswig; these, being only occasionally found south of the Eider and north of Flensburg, must be regarded as strictly local types. They are globular with a low rim. Urns of these types are found in the latest (5th and 6th century) cemeteries of Angeln. Corresponding types of urns have been found in English cemeteries (PLATE II), representing the continuation here of those very types which ceased in Angeln[22]. It has already been shown that cruciform brooches, which are typical of the Anglian region in England, have their oldest prototypes in Schleswig-Holstein[23]. The archaeological evidence thus confirms the emigration to Britain which caused the Angeln region to become deserted in Bede's time. Bede knew the facts, and there is all the less reason to distrust his account in the fact that the Norwegian Othere (Ottar) who visited the country in the 9th century knew also of the tradition[24].

On the other hand, historical research has recently shown that the distribution of population as recorded by Ptolemy[25] is untrustworthy as evidence for a Central European home of the Angles. Nor does archaeology provide any evidence of connection between Central Germany and England during the 5th century. It does however show very conclusively that the dense population of Angeln migrated to England during the 5th and 6th centuries A.D.; so that the hypothesis that the Anglians came from Schleswig is securely based.

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[21] H. Jankuhn, *Nydam und Thorsberg*. Neumünster 1950, p. 27 ff.

[22] F. Tischler, ' Bemerkenswerte Funde der Völkerwanderungszeit im Ruhrmündungsraum '. *Probleme der Küstenforschung im südlichen Nordseegebiet* I. 3, I, 1942, pp. 84–95. p. 90, fig. 5 and p. 91, fig. 7 a.

[23] A. Plettke, *Ursprung und Ausbreitung der Angeln und Sachsen*. Hildesheim-Leipzig 1921, p. 8 f. A. Genrich, ' Formenkreise und Stammesgruppen in Schleswig-Holstein an Hand der geschlossenen Funde des, 3–6 '. *Jahrhunderts. Kieler Dissertation*, 1939. H. Jankuhn, ' Zur Frage nach der Urheimat der Angeln '. loc. cit., p. 41 ff.

[24] *King Alfreds Orosius*, edited by Henry Sweet, M.A. Part I, London 1883, p. 19.

[25] Th. Steche, loc. cit. G. Schütte, *Ptolemy's Map of Northern Europe*, 1917. G. Schütte, ' Anglernes angelske Hjemstavn stadfaestet '. *Aarbøger for nordisk Oldkyndighed og Historie*, 1943, pp. 207–11.

The Date of Caernarvon Castle

by A. J. TAYLOR

(*Inspector of Ancient Monuments for Wales*)

NEARLY forty years ago Sir Charles Peers read a paper of outstanding importance on Caernarvon Castle. The description he then gave of the various buildings of which the castle is composed is printed in the *Transactions of the Cymmrodorion Society*[1]. It will never be bettered, combining as it does a mastery of detail with a simple and lucid presentation of a most complicated structure. It is prefaced by an analysis of contemporary documents bearing on the original work of construction, on the gradual progress of building and on the castle's subsequent history[2]. On the strength of these records a concise building sequence is established[3], and this has in its turn been adopted as the basis of all subsequent writings on the castle[4]. A dated ground-plan, setting out this sequence in graphic form, accompanied the printed account, and has since been widely reproduced.

The purpose of the present review is to examine afresh certain of the early documents on which this currently accepted dating is based and to reconsider, where this seems desirable, the interpretations of them previously put forward. If, as will be suggested, some of these interpretations require modification, a major revision in the dating sequence will necessarily follow.

Attention has lately been given elsewhere to the earliest stages of building work at Caernarvon Castle, following the arrival of the English forces there in the spring of 1283[5], and it is not proposed to consider here how soon the initial timber structures then erected began to be superseded by buildings of stone[6]. Let us, instead, first examine the important memorandum which was added as a postscript to a letter sent by Walter of Hereford and Hugh of Leominster, master mason and chamberlain of Caernarvon respectively, to the treasurer and barons of the Exchequer on 25 February 1296. The interpretation of this short record is crucial in no less than three particulars, and it is necessary to consider it in the original[7]:

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[1] Session 1915–16 (henceforward cited as *T.C.S.*), pp. 28–69.

[2] ibid, pp. 1–28.

[3] In short, this is: S. and W. section 1285–91; S. and E. section 1295–1301; N. section 1315–22.

[4] e.g. Caernarvon Castle *Official Guide*, which has passed through two editions and many printings, and Sidney Toy's *Castles* (1939), p. 195.

[5] See the present writer's paper in *History*, Oct. 1950, ' The Birth of Edward of Caernarvon and the Beginnings of Caernarvon Castle ' (Historical Revision, No. CXVI).

[6] The point is discussed on pp. 45–6 of Dr J. G. Edwards's Rhys Memorial Lecture, ' Edward I's Castle-building in Wales ' (*Proc. Brit. Acad.*, XXXII, pp. 15–81), which appeared after this paper was written. Dr Edwards shows that, if due allowance is made for certain missing rolls of accounts, there is room for supposing that the stonework of the castle may well have proceeded much more rapidly than has hitherto been realized. That this was indeed the case is suggested by the fact that already in the third building season (1285), when Peers thought the main castle construction had scarcely begun, lead was being sent by sea from Bristol for roofing the castle buildings: in the Liberate Roll of 14 Edward I there is note of the payment of £31 7*s* 8*d* to Peter de la Mare, constable of Bristol castle, for 14 loads of lead bought and sent by him *usque castrum nostrum de Kaerenarvon ad domus nostras eiusdem castri inde cooperiendas anno regni nostri terciodecimo* (*P.R.O.*, Liberate Roll, no. 62).

> Memorandum quod in muro inchoato circa motam castri de Carnaruan sunt iiij turres inchoate, qui quidem murus continet in longitudine xviij perticatas ; et de istis perticatis viij pertice continent in altitudine xij pedes, et x pertice continent in altitudine xxiiij pedes ; et iste murus continet in spissitudine xv pedes.

First, to what period of time does the word *inchoato* (begun, in the sense of unfinished) refer ? Peers thought it meant work started in 1295 and carried to the stage described in the officials' letter by February 1296[8]. Another interpretation is, however, possible. As Peers himself rightly says, the earlier part of the letter shows that the summer months of 1295 were wholly given up to the task of rebuilding the partly destroyed town wall ; that this took until 10 September ; and that ' they then took up work on the castle till 27 November, *when building stopped for the winter* (my italics), and the workmen were employed on dressing freestone, working timbers, quarrying and carrying stone from the quarries, and forging the iron work[9]'. In other words there were, in 1295, only 2½ autumn months when the work carried out was mainly actual construction work on the castle. Even if we assume the necessary amount of stone for this had already been quarried and brought during the summer months, it seems at least doubtful whether this short period, perhaps only part of it good building weather, could have seen the erection of a wall 15 feet thick, an average of 18 feet high and (accepting for the moment Peers's interpretation of the figures)[10] 150 yards long, and incorporating the bases of four towers, presumably built to something like the same average height. It seems still less credible in view of the priority which the surviving accounts suggest was being given in the 1295 season to the labour requirements of the new castle rising at Beaumaris[11]. If these doubts are valid, it looks as if the *murus inchoatus* and the *quatuor turres inchoate* may actually have been begun before the disaster of October 1294[12]—which probably means before the end of 1287, the last previous year in which a substantial sum had been spent on the Caernarvon works[13]. In this case, the purpose of their mention in the schedule to the letter of February 1296, would be to set down a record of the point previously reached on the section of the castle on which it was intended to resume work at the commencement of the coming season. One cannot help wondering whether, if this section had really only been begun in the preceding September, (a) it would have been necessary to write such a note about it at all, and (b), if it was necessary, whether we might not have expected some such words as *in muro nuper incepto* rather than the vaguer *muro inchoato*. Possibly the figures were given in answer to an actual question from the Exchequer, who may have required an *aide memoire* as to what remained to be done to complete this part of the building; in the form in which they are stated they would provide an indication of the scale of the work to be undertaken, as well as of the extent of that previously done.

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[7] *P.R.O.*, *Exchequer Accounts*, 5/18, no. 11. The whole document has now been printed by J. G. Edwards, loc. cit., pp. 79–80 ; its implications are examined ibid, pp. 46–7.

[8] *T.C.S.*, p. 12 : ' The new work was evidently started in 1295, and by the end of the year had progressed as already described '.

[9] ibid, p. 10.

[10] ibid, p. 12, note 1.

[11] Works expenses at Beaumaris were £8555 7*s* 10*d* for one year (18 April 1295—7 May 1296) (Pipe Roll, no. 158), as compared with £4393 13*s* 7¼*d* at Caernarvon for four years (11 Sept. 1295—28 Sept. 1299) (ibid, no. 146, m. 24).

[12] J. E. Morris, *Welsh Wars of Edward the First*, pp. 242, 253.

[13] *Pipe Roll*, no. 136.

What of the phrase *circa motam castri*? Peers rendered it ' round the motte of the castle[14] ', and on the strength of this identified the building referred to with the southeast side of the castle, including the Northeast Tower, the Queen's Gate and the Black Tower.

It cannot be questioned that there was formerly a motte in the upper ward or outer bailey of the castle, dating from the late 11th century and largely surviving until after 1870 (PLATE IVb[15]). But it is to be doubted whether *circa motam castri* means ' round the castle motte ' or whether it would be written by a 13th-century clerk who wanted to say that; if he had used such a phrase at all he would have said simply *circa motam*, the addition of *castri* being in this context as tautological in Latin as that of the words ' of the castle ' in English. The true meaning of *circa motam castri* is, it is believed, something quite different. A clue to it is provided by a phrase in the nearly contemporary accounts for the works at Beaumaris, recording the payment of £1468 12*s* 0*d* wages *fossatorum faciencium fossata et motam ubi castrum de Bello Marisco situm est*, from April to September 1295[16]. At no time was there a motte at Beaumaris, and we may be sure the diggers of 1295 were not employed on building one. The *fossata* they dug were the foundation trenches for the towers and curtain walls, and the *mota* was the castle ditch or moat. And so it was at Caernarvon[17]. When this is the meaning of *mota*, *mota castri* is not tautological, for where there is a defended borough it may be used in contradistinction, expressed or implied, to *mota ville*. The *murus inchoatus circa motam castri* is thus not ' the wall begun around the motte ', but the ' unfinished wall along the castle ditch '. It is, in other words, the wall forming the north side of the castle, and the four *turres inchoate* are the Well Tower, the Granary Tower and the towers flanking the King's Gate. It has always been a difficulty with the old theory that the section of wall to which Peers thought the description referred contains six towers and not four and that at best it extended only halfway round the motte.

Another, more serious, objection arises from the measurements. Being thus led to identify the *murus inchoatus* and its four towers as the section round the east end of the motte from the Northeast Tower to the angle west of the Black Tower, Peers says that ' its length of 18 perches may be taken as 450 feet; the perch in 1316 being said to contain 25 feet. Measuring round the faces of the towers and the walls, on the outside, from the Northeast Tower to the bend in the south curtain, this figure is approximately correct[18] '. This means, however, that in order to arrive at a figure which for the purposes of the required identification is only approximately correct, an abnormal and exceptional reckoning of the linear perch has to be adopted, the standard perch being one of 5½ yards, i.e. 16 feet 6 inches, not 25 feet. Certainly there is, as Peers says, a reference to a perch of 25 feet at Caernarvon in an account for work done on the quay there in

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[14] *T.C.S.*, pp. 11, 12.

[15] I have to thank Mr W. J. Hemp for calling my attention to this photograph and allowing me to reproduce it here. Plates II and IVa are from photographs by Mr G. B. Mason of the National Buildings Record, to whom thanks are due for permitting their reproduction. Plates I and III are from photographs by the author. The plan (p. 31) has been redrawn from a survey in the possession of the Ministry of Works.

[16] Pipe Roll, no. 158.

[17] Indeed the word is actually used of the Caernarvon castle ditch in a record of June 1283: ' . . . pro fossur' et apparatu noue mote de Caernaruan . . . ' (*Exch. Accts.*, 351/9).

[18] *T.C.S.*, p. 12, note 1.

November 1316[19]. But the fact that in this instance the clerk goes out of his way to define a length for the perch at all suggests that it was not the ordinary reckoning, which would surely have needed no such definition[20]. There would seem to be little justification for applying it, without considerable reserve, to the memorandum of 1296. On the contrary, the absence of a qualifying figure in that document leads one to suppose that the 18 perches of the *murus inchoatus* were standard perches, giving a length not of 450 but of 297 feet.

If we look carefully at the wording of our record, we see that the measurements of length, height and breadth which it gives relate only to the *murus* and not to the *turres*. There is thus a double error in an interpretation which besides increasing what is likely to be the true length by 153 feet accounts for that increase by ' measuring *round the faces of the towers* and the walls ' ; and the fact that the result is ' approximately correct ' can only be regarded as a coincidence. Let us now, therefore, omit the towers and take instead strict measurements of the lengths of wall bordering the castle ditch, and see what light they throw on our view that this, and not the wall surrounding the south and east sides of the site of the motte, is the *murus inchoatus circa motam castri . . . qui . . . continet in longitudine xviij perticatas*. Each section of wall is measured between the points at which its outer face is cut by the outer face of a contiguous tower, the measurements being given to the nearest six inches. Taking the Eagle Tower as our starting point, we have a length of 119 feet from there to the west side of the Well Tower ; from the well-projection on the east side of the Well Tower to the west face of the King's Gate the length is 69 feet 6 inches ; from the east face of the King's Gate to the west side of the Granary Tower is 55 feet ; and from the east face of the Granary Tower to the Northeast Tower is a further 61 feet 6 inches. This gives a total wall length of 305 feet, which tallies closely enough with the 297 feet represented by a wall of 18 perches of 5½ yards each. The slight discrepancy may arise from the fact that, inwards from both the Eagle and the Northeast towers, a little curtain walling had already been built in continuation of those structures (PLATES IIa and IIIa), to a greater height in the former case than in the latter. Beside the Eagle Tower this return wall terminated on a sloping line, and we cannot now be sure at exactly what level the 1296 measurements were taken. But the figures are, it is suggested, sufficiently close to afford conclusive evidence that the wall *circa motam castri* is that fronting the castle ditch and linking the two ends of the town wall[21].

We may now consider the supposed change of build marked by the shift in the line of the south curtain midway between the Black Tower and the Chamberlain Tower. According to Sir Charles Peers, this turn in the wall represents a break between work built before 1291 (to the west) and work built after 1295 (to the east)[22]. Nevertheless it

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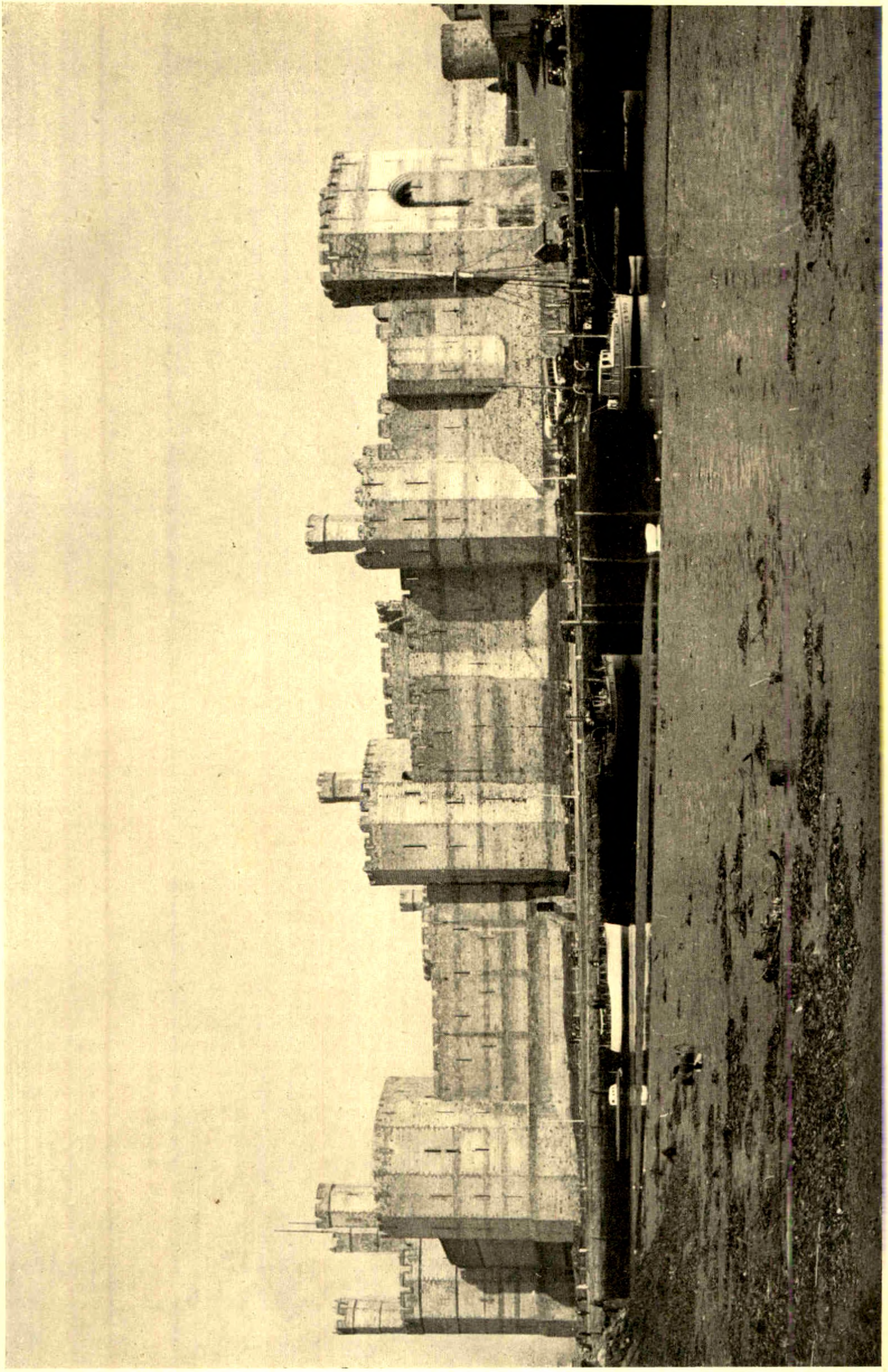
[19] *Exch. Accts.*, 486/29 : ' Henrico de Elreton cementario pro duodecim perticatis Kaye ville de Kaern' ad tascam faciendis de petra et calce sumptibus suis propriis, percipienti pro perticata continente xxv pedes viij.li, . . . in partem solucionis iiij.xxxvj.li. quas percipere debet pro tasca predicta—lx.li. ' Also ' Ricardo de Wykombe pro quinque perticatis, dimidio rod', tribus pedibus Kaye predicte cum arrena inter murum ville et dictam kayam obstupandis, qualibet perticata continente xxv pedes . . . xl.s. '

[20] Possibly a long perch was allowed because a water wall presented additional difficulties. Dr. J. G. Edwards draws my attention to the fact that calculations based on payments made in July 1279 for building the side of the castle ditch at Flint (*Chancery Miscellanea*, 2/2/17) point to a perch of 25 feet. This was probably a wet ditch and similar considerations may have applied.

[21] It is not practicable to base any arguments on the heights to which the wall is described as standing in 1296, as the depth of the foundation from which these were presumably taken is unknown.

[22] *T.C.S.*, pp. 13, 59.

PLATE I

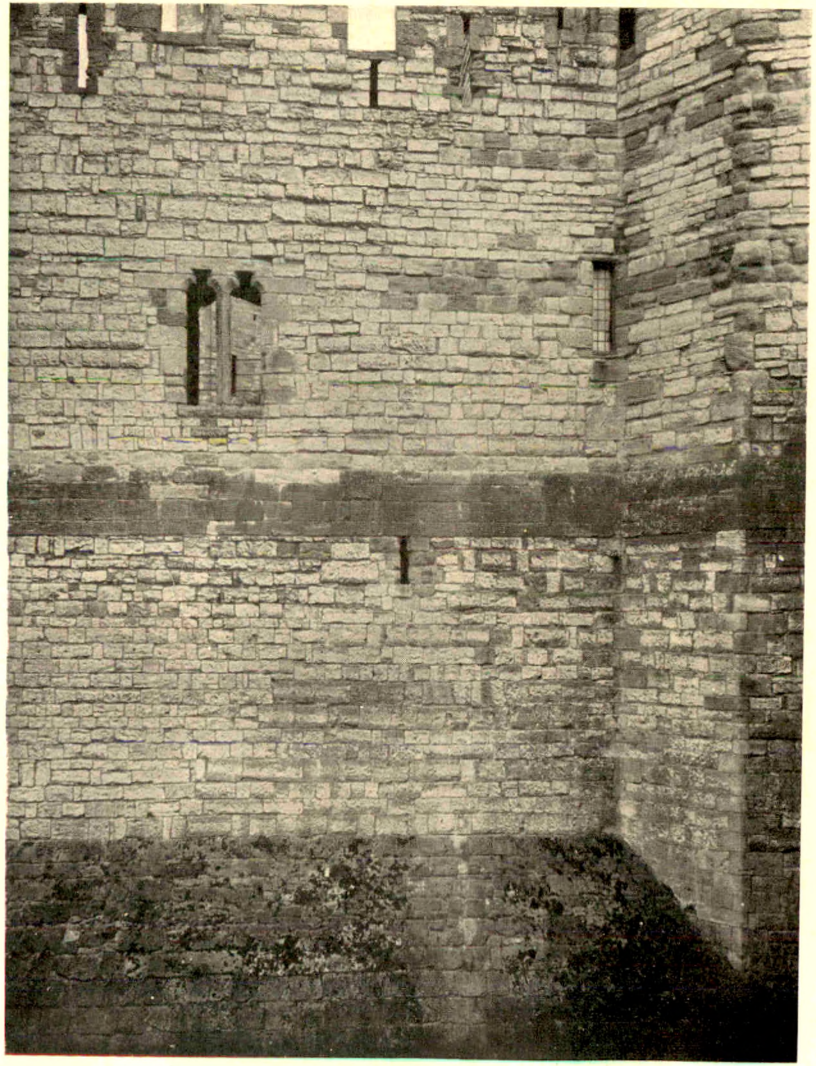


CAERNARVON CASTLE: THE SOUTHERN FAÇADE FROM ACROSS THE RIVER SEIONT

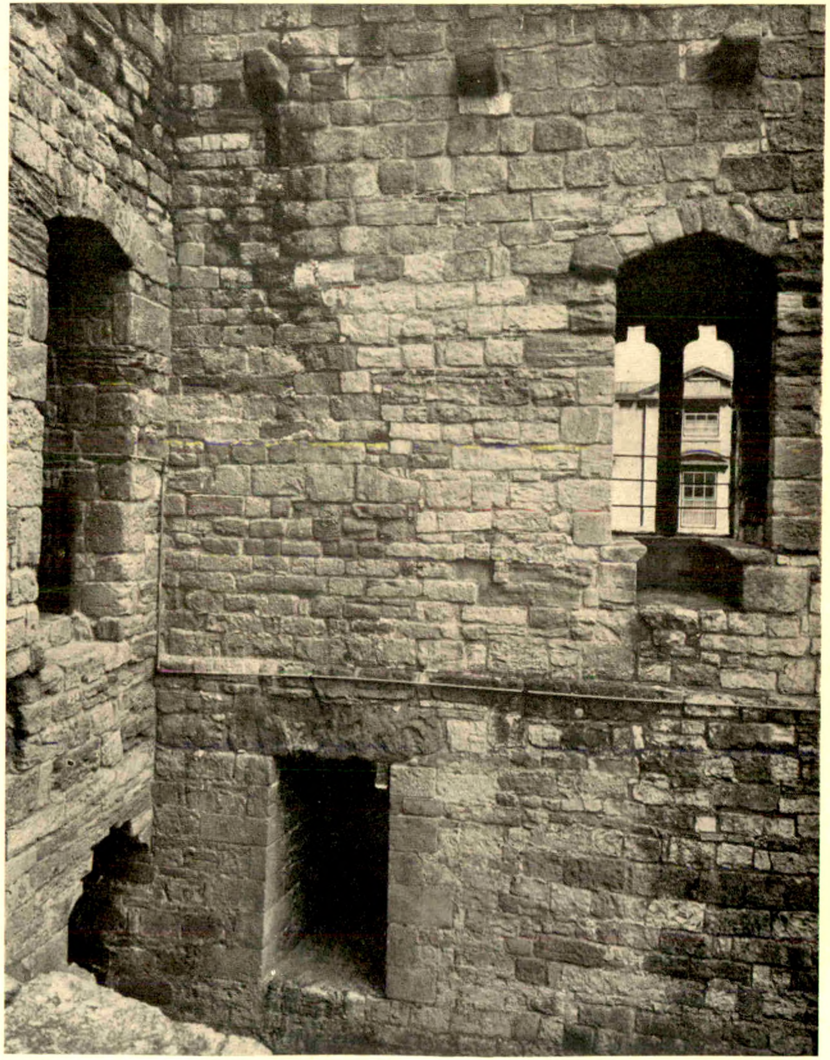
Ph. A. J. Taylor

facing p. 28

PLATE II

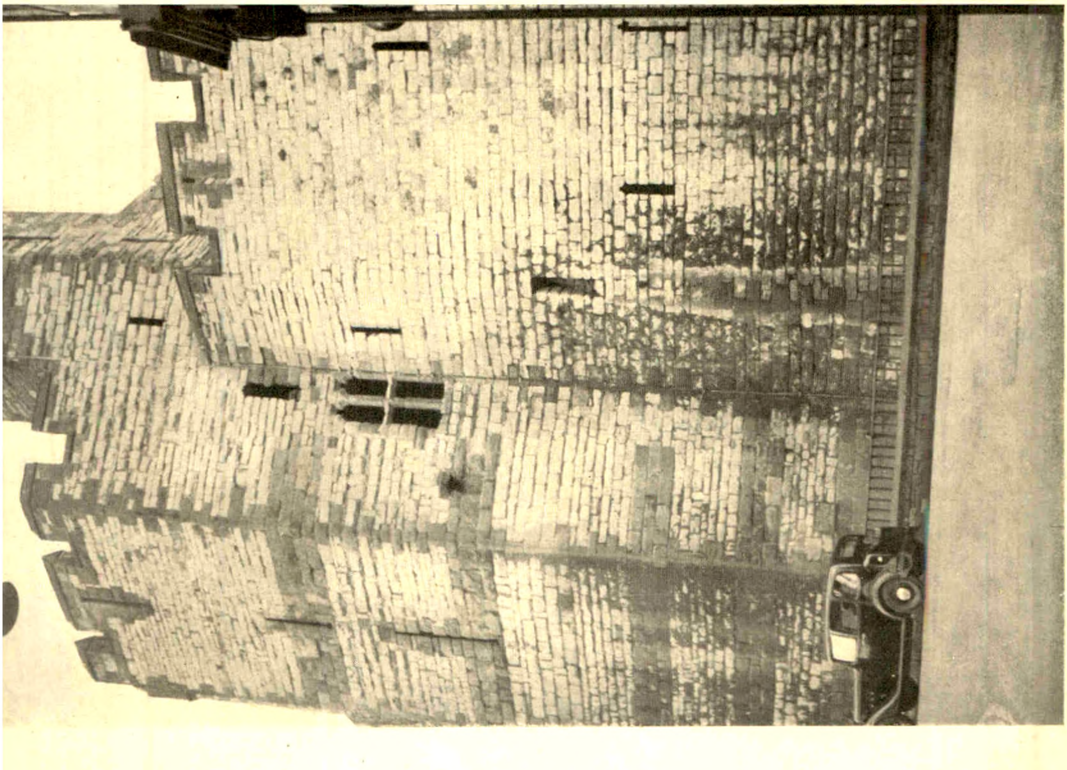


a. CAERNARVON CASTLE: JUNCTION IN CURTAIN WALL EAST OF EAGLE TOWER (NORTH FACE OF WALL)

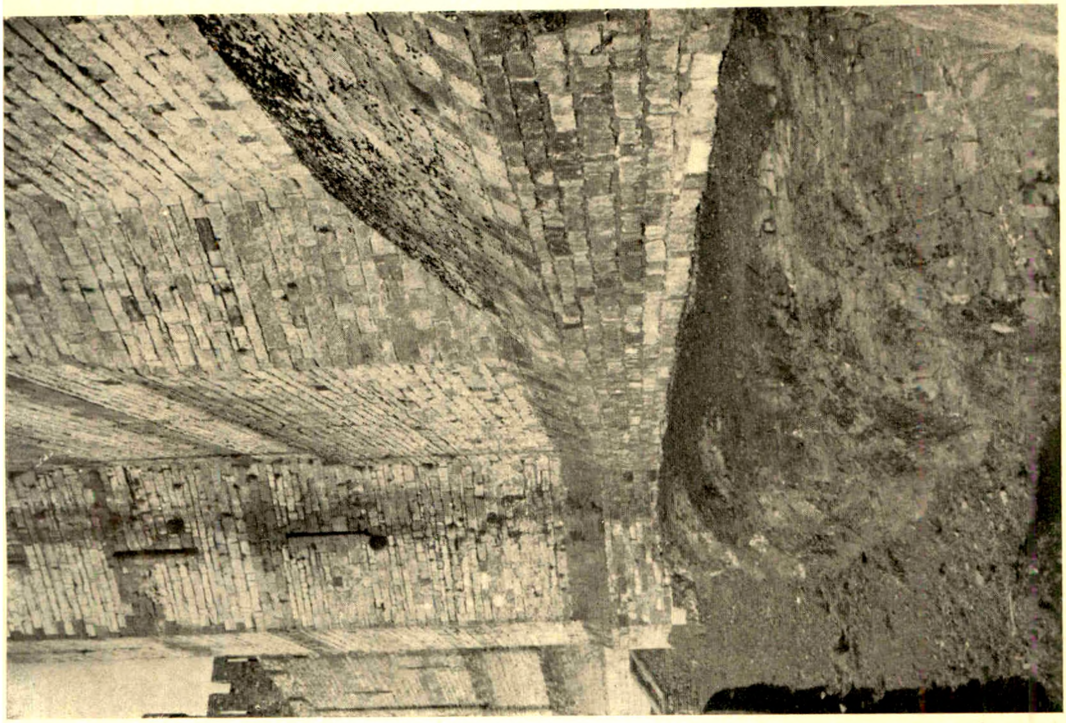


b. CAERNARVON CASTLE: JUNCTION IN CURTAIN WALL EAST OF EAGLE TOWER (SOUTH FACE OF WALL)

Phs. G. B. Mason. *Copyright,* National Buildings Record

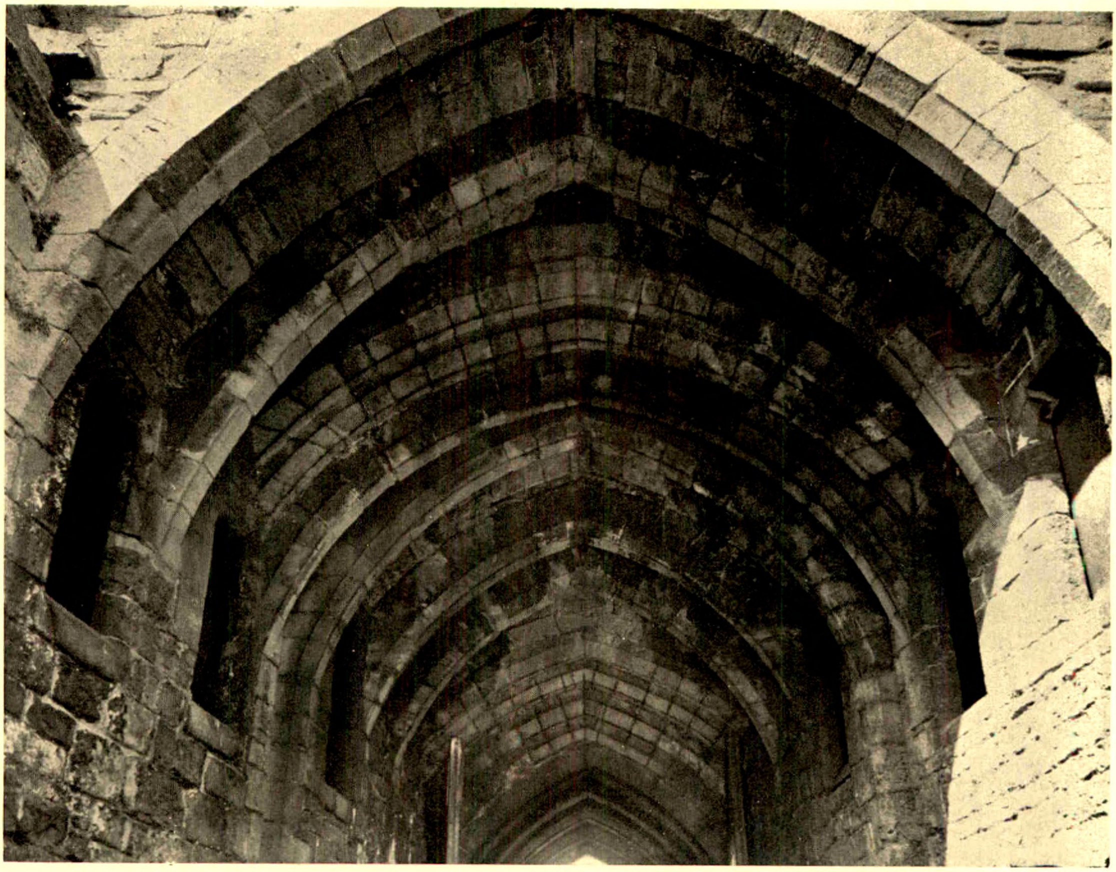


a. CAERNARVON CASTLE: JUNCTION IN CURTAIN WALL WEST OF NORTHEAST TOWER

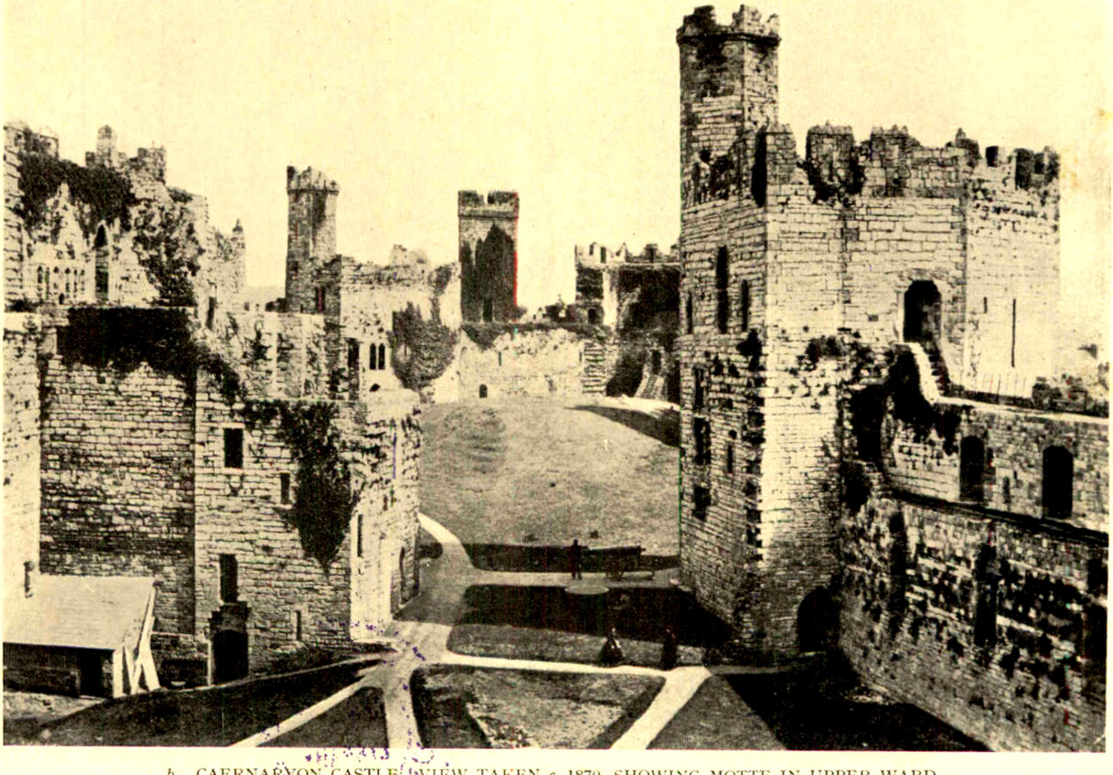


b. CAERNARVON CASTLE: SOUTH CURTAIN WALL BETWEEN BLACK AND CHAMBERLAIN TOWERS

Phs. A. J. Taylor



a. CAERNARVON CASTLE: VAULTING OVER PASSAGE OF KING'S GATE
Ph. G. B. Mason. *Copyright*, National Buildings Record



b. CAERNARVON CASTLE: VIEW TAKEN *c.* 1870, SHOWING MOTTE IN UPPER WARD is impossible not to be struck by the contrast between the form of this ‘ break ’ and that of the marked changes of build, already briefly referred to, which occur at either end of the north front. At the west end, a few feet from the Eagle Tower, the external decorative bands of Aberpwll stone terminate, and the regular coursing of the facework, continued evenly thus far from tower to curtain, is interrupted throughout its height (PLATE IIa). The change at this point is no less noticeable on the inner side of the wall (PLATE IIb). At the east end, beside the Northeast Tower, there is another break characterized by similar features. A little to the west of the tower there is a slight angle in the wall, the break at ground level coming immediately west of this ; the sloping apron at the base of the wall, which is continuous round the east end of the castle, here stops abruptly, while a little higher up there is an equally abrupt finish, on the line of the quoin, to the lowest of the three-course Aberpwll bands (PLATE IIIa). There can be no doubt that these irregularities, both east of the Eagle Tower and west of the Northeast Tower, reflect an appreciable pause in the building operations, and it is clear from the way the masonry slopes that the north wall was completed against masonry already existing to the east and west of it, and not vice-versa. When, however, we come to look at the change of alignment between the Black Tower and the Chamberlain Tower we are confronted with something wholly different. On either side the facework is alike ; the decorative banding is continued regularly (PLATE I) ; internally there is no break or step in the level of the wall passages, such as we might have expected if the changed alignment masked a ‘ join-up ’ of two different building periods ; and, lastly, when we examine the foot of the wall on the outside we find that its seven lowest courses follow a perfectly straight alignment from tower to tower (PLATE IIIb). It therefore seems to be open to question whether the change of direction taken by the wall above this level affords any evidence whatever of differing dates of construction. If it does not, there must be an alternative explanation, and it is not far to seek. The wall at this point may be presumed to be built on the site of the ditch of the former motte. The sloping apron which begins here (PLATE IIIb) and reinforces the base of the curtain all round the east end of the castle was introduced to revet and contain ground, the stability of which was weakened by its artificial nature. The surviving short foundation, which can be seen within the castle pointing across the upper ward in the direction of the King’s Gate, shows that the Edwardian builders intended to complete this reinforcement by circling the inner as well as the outer side of the mound with a containing wall[23]. It looks very much as if the twist in the south curtain relates only to the existence of the former ditch round the motte and to the planned junction with this future cross wall. It appears to provide no evidence which can properly be regarded as indicating a halt and subsequent resumption of building.

This is a convenient point at which to summarize the argument and note its implications. We have suggested

(i) that the wall described as *inchoatus* in the schedule of February 1296 is unlikely to have been constructed to the stated dimensions in the short period available in 1295, and must in all probability have been commenced at a date not later than 1287 ;

(ii) that the wall in question is that forming the north side of the castle, containing the Well Tower, the twin towers of the King’s Gate and the Granary Tower ; and not, as hitherto held, the wall forming the east and southeast side of the castle, containing the Black Tower, the Queen’s Gate and the Northeast Tower ;

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[23] The revetting suggests the possibility that the original intention may have been not to remove the motte, but rather to put a heavy building on it. A tower in this position would have been the crown of the whole design.

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(iii) that the measurements given in the schedule represent standard perches of 5½ yards, and that on this basis 165 feet of the north curtain had been built to a height of 24 feet from the foundation, and the remainder to a height of 12 feet, by 1287, or at latest by 1292, when the first series of works payments cease.

(iv) that there is no essential difference in building date between the sections of the south curtain to east and west of the change of alignment between the Black Tower and the Chamberlain Tower ;

(v) that there are thus only two, not three, constructional breaks in the building, viz. to the east of the Eagle Tower and to the west of the Northeast Tower, the lowest portion of the intervening curtain being contemporaneous, to a height of at least 12 feet from the foundation, with the rest of the enceinte.

Taken in relation to each other, these considerations imply :

(i) that the whole of the castle outside the ends of the town wall, i.e. from and including the Eagle Tower, round the south side as far as and including the Northeast Tower, had reached an advanced stage by 1292[24] ; for the two ends of this section are demonstrably not later than the intervening north curtain, which, if our interpretation is correct, had itself been built to a height of from 12 to 24 feet by that year ;

(ii) that the conception of the building as falling into three sharply defined periods, viz. south and west section 1285–91, south and east section 1295–1301 and north section 1315[25]–22, cannot be sustained ;

(iii) that the castle is on the contrary a single design conceived as a unity at the time work was begun on it in 1283–4, and, except on the north side, carried a considerable way towards completion within the succeeding three or four years ;

(iv) that the growth after 1296 was essentially an 'upward' growth, with the gradual addition of height to work already well started in varying degree round the whole circuit of the castle, and not, as has hitherto been held, an 'outward' growth, with the successive addition of entirely new sections to a first block which had consisted only of the lower storeys of the Eagle Tower and the buildings immediately to the south and southeast of it[26] ; this 'upward' growth was most marked on the north side of the castle, where prior to 1292 building had not exceeded a height of 24 feet above moat level.

The above propositions are embodied in the accompanying ground plan (opposite).

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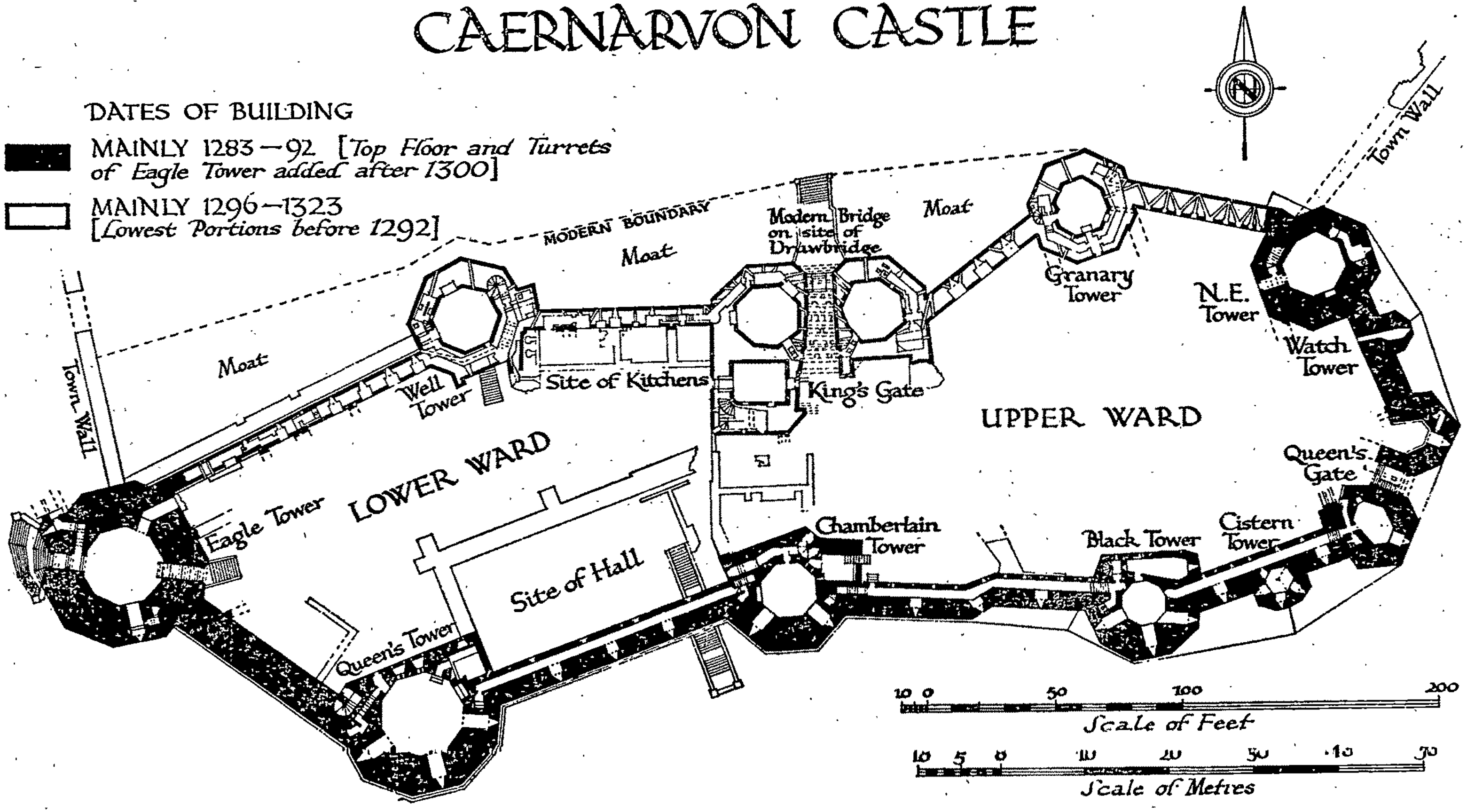
[24] The three lower storeys of the Eagle Tower, which are earlier than the fourth storey and surmounting turrets (*T.C.S.*, p. 46), may represent the general height reached by the south curtains and buildings ; the fact that the hall had been built to gable height before the time of Madoc's revolt (cf. ibid, p. 36) shows there must have been at least equivalent height in the section of the curtain against which it was built. The uppermost part of the curtain between the Eagle and the Queen's towers, the beginning of which is marked by a string course, was probably added when the Eagle Tower was heightened, i.e. after 1295.

[25] There is in any case no particular virtue in this date. Work was certainly going on in 1304–5 (see below, p. 33). I have shown elsewhere (*Trans. Caernarvonshire Hist. Soc.*, 1948, pp. 16 ff.) that it was also in progress in 1309; and that it was in that year, not 1315, that Walter of Hereford died and his deputy Henry of Ellerton succeeded him as master mason at Caernarvon.

[26] This reading of the building is at any rate partially supported by documentary evidence for the completion of the heightening of the Eagle Tower and another tower in about 1316, for the erection of the statue over the King's Gate in 1320, and for work on the hall over that gate in 1321–2 (*T.C.S.*, pp. 16–18).

CAERNARVON CASTLE



It is advisable that we should consider this theory of the building development of the castle in relation to such works accounts as are preserved on the Pipe Rolls[27]. Our view, based as it is on what seems to be the evidence of the buildings themselves and on the note of February 1296, would of course be unacceptable if it were found to be incompatible with the expenditure there recorded. While it is difficult to make reliable comparisons, and easy perhaps to draw parallels that seem closer than they may really be, with contemporary works at other castles, we must nevertheless make the attempt if the Pipe Roll accounts are to throw any light on our problem. Let us, therefore, examine the figures for Conway and Harlech, and compare the achievement which they can be seen to represent with the corresponding figure and suggested achievement at Caernarvon. The total ' works ' and related expenditure given in the Pipe Rolls for the three places is as follows :

Caernarvon	£6566 3*s* 0½*d*.	(Jan. 1283–Oct. 1290[28]; mostly before Sept. 1287).
Conway	£7714 11*s* 10½*d*.	(Nov. 1284–Sept. 1289[28]; mostly before Oct. 1287).
Harlech	£8178 5*s* 7*d*.	(May 1285–Dec. 1290[28]; mostly before Dec. 1289).

According to the hitherto accepted dating, the only part of Caernarvon castle built during the period covered by the first of these figures (which it must be remembered includes also the cost of the town wall and of work on the quay) is from the Eagle Tower southwards to a point east of the Chamberlain Tower. The question we now have to ask ourselves is whether it is reasonable to suppose that the figure could also have included the cost of the unfinished Queen's Gate with the buildings on either side of it from the point east of the Chamberlain Tower to the break west of the Northeast Tower, together with the lower 12 to 24 feet of the northern towers and curtains. At first sight it may seem improbable that so much could be done for £1150 less than it cost to build Conway and nearly £2000 less than it cost to build Harlech. But this is not so. The Conway figure covers virtually the whole of that great castle as we now see it, together with a town wall more than twice the length of that of Caernarvon and originally containing 24 towers and 3 twin-towered gateways, as compared with the Caernarvon wall's 8 towers and 2 gateways. The greater cost of Harlech (which of course also covers the whole castle except the Water Gate and the outer enclosure of the castle rock[29]) is to be accounted for by the fact that, being ' concentric ' in plan, it has an additional ring of towers and curtains which is absent at Conway and Caernarvon; by the great size of its three-storey keep-gatehouse; by the expense of digging its wide rock-cut ditches; and by the relatively higher charges for carriage and materials arising from its more remote situation. If these factors, and particularly the imposing scale of the completed works at Conway and Harlech, are taken into consideration, it does not seem that our interpretation of the building sequence at Caernarvon places an undue strain on the Pipe Roll figures for the 1280s. With regard to the figure of £4393 13*s* 7¼*d* recorded as having been spent on works at the castle from September 1295 to September 1299[30], this must be presumed to have included not only much work on raising the *murus inchoatus* and its towers and elaborate gate (PLATE IVa), but also most of the rebuilding and refitting of the pre-1290 structures which had been sacked by the Welsh in the winter of 1294–5.

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[27] *Pipe Rolls*, nos. 131, m. 26d, and 136, m. 28.

[28] The dates are those of accounting periods within which payments were made. There is evidence that at Conway and Harlech works were in progress earlier than the commencing date of the account, while at Caernarvon the figure here given actually only covers the work from August 1284 onwards although it had then been in progress for more than a year.

[29] Cf. the present writer's note, ' Harlech Castle: the dating of the outer enclosure ', in *Journal of the Merioneth Hist. and Record Soc.*, 1951, pp. 202–3.

Finally, it is to be noted, the statement on p. 6 of Peers's article that expenditure of £582 12*s* 8¼*d* was incurred on the castle in 1288–9 requires correction, in that the account in question dates from 1304–5[31]. This is important for two reasons. It shows that work was going forward fairly steadily between 1301 and 1315, years which are a blank period in Peers's scheme of dating[32]. Secondly, the true dating of this account removes the only evidence that had ever been produced for naming Walter of Hereford as master mason at Caernarvon prior to 1295. There is in fact no documentary record to connect this well-known English builder with the Caernarvon works until 30 June of that year[33]. There is, on the other hand, precise evidence to show that he was master of the works at Vale Royal abbey in 1287, and also that he still held that post and received allowances *pro robis* in respect of it during the year ending 19 November 1290[34]. It would perhaps not have been altogether impossible for Walter of Hereford to be master mason at two such important works as Vale Royal and Caernarvon simultaneously, but the obvious difficulties of such an arrangement appear on the whole to make it unlikely. It seems unlikely, too, that the Wardrobe Book of 18 Edward I would have referred to Master Walter simply and solely as 'cementarius, magister operis cementarie ecclesie Abbath' de Valle Regali[35]' if he were in fact at that time already employed as the master of the works at Caernarvon. The evidence is not as explicit as we could wish, but the weight of it seems to be against Walter's being connected with Caernarvon during the creative and formative years between 1283 and 1292 when, as we believe, the foundations of the *whole* castle were laid and the entire southern façade carried a long way towards completion. If, then, we eliminate Walter of Hereford from being concerned with the building before 1295, the records name only one other to whom the architectural creation of the castle can be attributed. This is Master James of St. George, the *magister operacionum regis in Wallia.*

In a reference to this problem published in 1950, but written in 1947 when the author was unaware of the Vale Royal evidence of 1287 and 1290, the suggestion was made that Walter of Hereford might have been responsible for the essentials of the design of the castle: for Caernarvon's octagonal towers give it an appearance which contrasts in marked degree with that of other castles built under James of St. George's direction, and not least with that of the town wall of Caernarvon itself[36]. But it is perhaps a poor tribute to a great architect to assume that all his elevations must needs have followed similar patterns. That St. George was a versatile designer appears from the treatment of the Great Tower at Flint, a structure to which a true English parallel has yet to be found. It may well be that the real explanation of Caernarvon's distinctive characteristics is not that they reflect the mind of a different building master, but that they are a deliberate and conscious adornment, introduced at the royal will, of a building intended from its inception to become the principal seat of English power in North Wales and the capital of the new administration promulgated at Rhuddlan in 1284. Moreover, whatever may be the truth of some of the traditions about the early days of the young prince Edward, traditions which cannot now be traced further back than David Powel's 16th-century *Historie of Cambria*[37], there is at least some contemporary evidence that Edward I was not insensitive to the symbolic importance of the Gaer in Arvon or uninformed about survivals there of a long-past history[38]. Such considerations may have helped to influence the king to make the castle of Caernarvon an even more splendid and regal building than the others. Had events turned out differently it might have been destined to become the official palace and residence in Wales of a line of English princes, and in 1284 such a possibility was perhaps not without a place in the king's thought. But when we look beyond the superficial variations and more lavish detail which set the castle apart from its contemporaries, we find many features which have close parallels in the ' round tower ' castles. Thus in essentials the King's Gate would, when completed, have been just as much a building of ' keep-gatehouse ' type as those, for example, at Harlech or Beaumaris. The wall passages, with their flat shouldered vaults, are almost duplicated by those of Beaumaris, where we know James of St. George directed the works. Or again, the turrets which rise above the roofs of the towers inevitably recall those of Conway and Harlech, and similar turrets were intended at Beaumaris. There is close resemblance also between the pointed barrel vaults of the smaller Caernarvon rooms and those found in the other castles. It does not seem improbable, therefore, that the principal architect may have been the same man as directed the building of the rest of the castles in the group, but that in the case of Caernarvon the terms of his commission required a design that would be out of the ordinary.

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[30] *Pipe Roll*, no. 146.

[31] The error arose from a wrong dating in the printed *P.R.O. List of Exchequer Accounts, Various* (1912), where the roll (486/1) was given as 16–17 Edward I; in the Search Room copies this has been corrected to 32–33 Edward I. Cf. Edwards, loc. cit., p. 49, note 2.

[32] Cf. note 25 above. [33] *Calendar of Chancery Warrants*, I, p. 63.

[34] See my note on 'The Cloister of Vale Royal Abbey' in *Journal of the Chester and North Wales Archaeological, Architectural and Historical Society*, 1949, pp. 295 ff.

[35] Ibid, note 4. [36] *English Historical Review*, LXV, pp. 441–2.

Such an explanation would be consistent with such certain knowledge as we have of Master James's and Master Walter's appointments and movements. James of St. George, the senior in rank, was first and foremost a military architect and is named in that capacity as master of the king's works in Wales, including Caernarvon, in the vital period from 1284 onwards. Walter of Hereford's experience appears, on the other hand, to have been primarily ecclesiastical, with work at Winchcombe and Vale Royal abbeys occupying him up till at least 1290, and first coming to take charge at Caernarvon in 1295. If, as we argue, the whole design of Caernarvon had by then been conceived and work had proceeded to different stages over the whole area of the castle, there is nothing unlikely about fetching in the church-builder to execute the repairs and carry on with the work already planned, while the master castle-builder is engaged at full power on the creation of the brand new fortress at Beaumaris. It is more difficult to explain the absence of the master of the king's works from Caernarvon after 1295 if, as the old theory of development proposed, whole sections of the castle still remained to be erected from the ground.

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[37] Hilda Johnstone, *Edward of Caernarvon* (Manchester U.P., 1946), p. 7, note 1. The survival of the Caernarvon-born prince and the death of his elder brother barely four months later must be regarded as fortuitous; but one is inclined to wonder whether tradition may not preserve a genuine memory of a policy, conceived with the passing of the young Alphonso, of making capital out of the fact that the eldest surviving son was born in this ancient heart of Gwynedd.

[38] *Flores Historiarum* (Rolls Series), III, p. 59: (s.a. 1283) Apud Kaernervan, corpus maximi principis, patris imperatoris nobilis Constantini, erat inventum, et rege jubente in ecclesia honorifice collocatum. As the late Dr Charlesworth noted (*The Lost Province*, p. 28, note 2), Nennius had already recorded the existence at Segontium of an inscribed tomb of Constantine. Cf. also *Mabinogion* (Everyman edn.), p. 86, and C. E. Stevens in *Archaeological Journal*, XCVII, p. 134.

Radiocarbon dating results from the Old World

by C. B. M. McBurney
(*King's College, Cambridge*)

THE latest complete list of radiocarbon readings received from America brings the total number of dated sites and objects to upwards of 160. Of these 120 are from localities in the New World, and their archaeological implications have been reviewed by G. H. S. Bushnell in the last number of ANTIQUITY; the object of the present article is to do the same for the 40 odd results available from the Old World.

One of the most interesting features of these Old World results is the opportunity they offer to make a direct comparison between particular carbon readings and dates fixed by historical deduction, or ultimately based on Baron de Geer's system of varve chronology for the Post-Glacial period. Among the more practical issues of importance to archaeologists that we may hope to see clarified in this way is a closer estimate of the relative accuracy of different kinds of organic materials, and the exact contribution of such sources of contamination as are likely to be met with in the field.

As yet progress towards these goals is limited, though some of the more general features in the situation are already beginning to appear fairly clearly. At the same time it will be seen that a number of important questions still remain unanswered.

The position can perhaps be most conveniently appreciated in a graphical presentation such as that provided by FIG. 1. The horizontal coordinates are those of dates predicted on historical or other general grounds, with a rough estimate of the possible margin of error shown by the length of the horizontal line attached to the symbol. The corresponding vertical lines show the position and margin of error of the carbon dates. This margin of error is that referred to in the previous article, indicating that there is a three-to-one chance of the true value of the sample measured falling within it: the chances of the true value falling within twice that margin, it may be recalled, are about twenty to one.

The outstanding results suggested by this analysis would seem to be the following.

First as regards materials, the superiority of charcoal over most other materials (as predicted by the physicists) is becoming increasingly apparent. Moreover apart from the order of magnitude, there is a distinct suggestion that the type of error noted in readings based on wood or charcoal differs from that offered by materials such as peat or plant-bearing muds. Whereas in the case of peat it begins to look as if the chances of a date so fixed falling above rather than below the true value are about equal, with charcoal and wood all the important errors seem to fall below. Since it is understood that there is no physical or chemical reason why charcoal and wood should provide a later reading than any other substance, it would seem to follow that here the main source of error is in fact provided by contamination with more recent organic substances. During the past season the writer has been engaged in collecting carbon samples from prehistoric sites in Libya, and was impressed by the frequency with which fine hair-like rootlets of living vegetation are found in charcoal layers, particularly in caves and rock-shelters. These rootlets are often found a most surprising distance from the nearest living plant, and have an astonishing capacity to penetrate through rock-like layers of stalagmite. Apart from the percentage of new living matter thus introduced into the ancient samples, it is reasonable to assume that the presence of the rootlets promotes other forms of biological activity ultimately drawing their carbon from the surrounding atmosphere. On the other hand it is also true that *occasionally*, under favourable conditions, deposits—

particularly those relatively poor in organic matter—may be found fairly close to the surface and yet appear exempt from this source of contamination. It may be that it will ultimately prove more satisfactory to collect carbon from such relatively poor formations

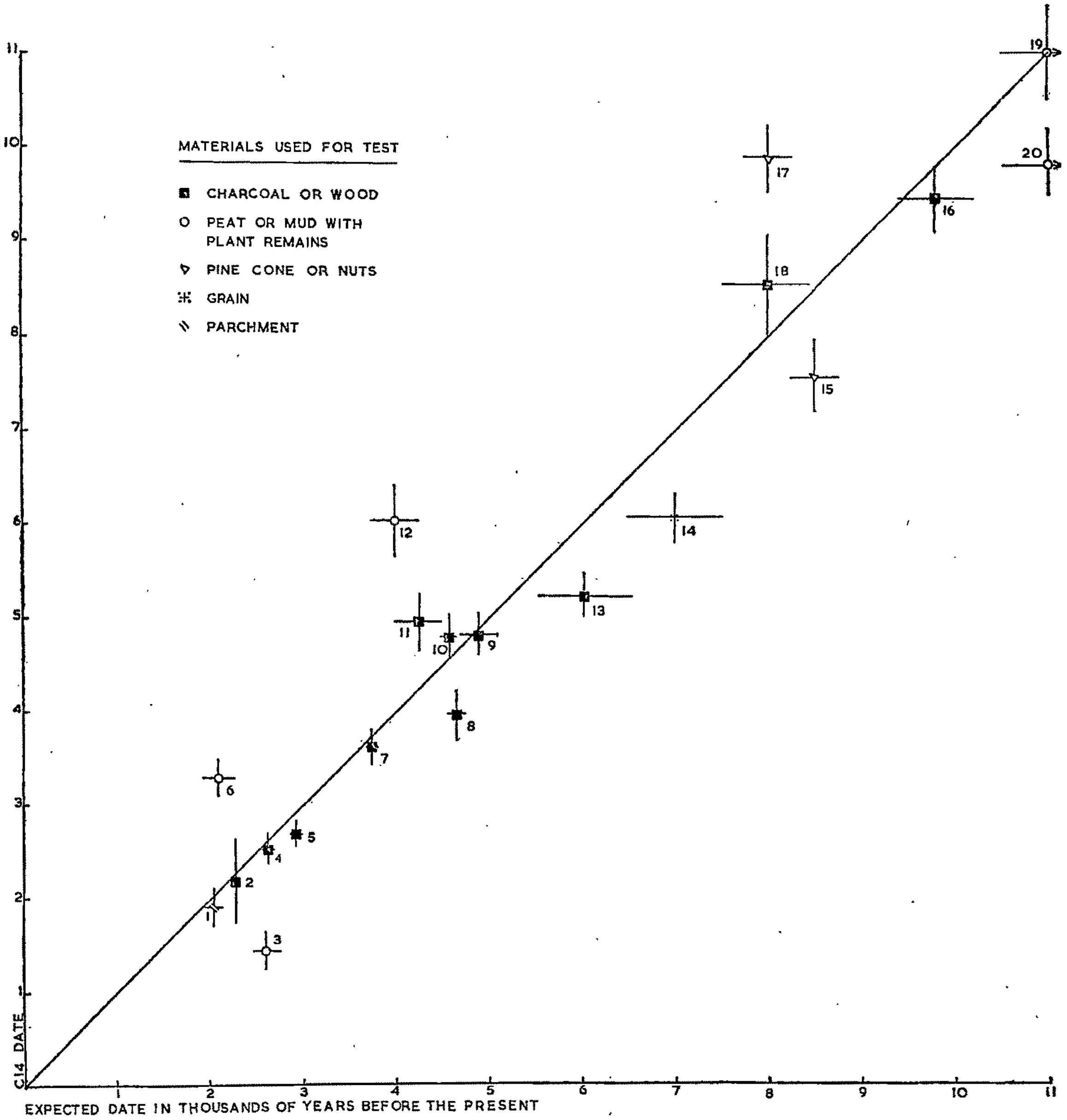

Fig. 1

(using perhaps some chemical means for separating the charcoal from the matrix) rather than from more obviously carboniferous remains such as actual hearths.

On the whole, though theoretical considerations would suggest a somewhat greater

[*continued on p.* 38

RADIOCARBON DATING RESULTS FROM THE OLD WORLD

The graph (FIG. 1) shows the relationship between radiocarbon dates and those predicted by historical and geochronological methods.

1. AIN FASHKA, PALESTINE (biblical scrolls).
2. EGYPT (Ptolemaic wooden coffin).
3. MELBACK, GERMANY (peat from *grenz-horizont*).
4. TAYINAT, SYRIA (wooden floor of Syro-Hittite palace).
5. Heart-wood of Sequoia.
6. SHAPWICK, ENGLAND (peat from zone believed to belong to Iron Age to Roman times).
7. EGYPT (fragment of funerary ship of SESOSTRIS III).
8. ,, (wooden beam from tomb of ZOSER).
9. ,, (beam from roof of tomb of HEMAKA).
10. ,, (beam from tomb of SNEFERU).
11. EHENSIDE TARN, CUMBERLAND (charred wood, associated with Neolithic A pottery).
12. SHAPWICK, ENGLAND (peat attributed to the neolithic).
13. EL OMARI, EGYPT (charcoal from Middle Predynastic settlement).
14. FAYOUM, EGYPT (grain from Neolithic A settlement).
15. AAMOSEN, DENMARK (pine cone from Boreal horizon).
16. STAR CARR, YORKSHIRE (wooden log from pre-Boreal horizon).
17. AAMOSEN, DENMARK (nuts from late Boreal layer).
18. ,, (late Boreal charcoal)
19. HANNOVER, GERMANY (mud from pollen-zone II).
20. HAWKS TOR, CORNWALL (peat from pollen-zone II).

margin of variability, shells would seem to be preferable from certain points of view. Thus, providing they are in a perfectly fresh condition and unaffected by any kind of chemical disintegration, they are clearly easier to free from recent remains. The actual results so far obtained with shell are as yet insufficient to confirm this, though it will be a matter of some practical importance to archaeologists working in coastal areas to see how far they do so in the future.

Little use of burnt bone has yet been made in even approximately datable contexts, though here again we are warned by the physicists to expect even greater errors than with shell. It is true that a reversed dating has been recorded for successive layers in the newly discovered Belt Cave in Iran[1], but it is perhaps not fair to attach too much importance to this in view of the small size of the samples submitted for examination, and the consequently extremely large margins of error in the observations. Turning from these general considerations to some of the more interesting particular results, the following comments may be offered.

The most interesting date at the lower end of the series, is probably that assigned to the newly discovered biblical manuscripts from Ain Fashkha, in Palestine. It will perhaps be recalled that the extreme dates assigned on palaeographic and literary grounds ranged over some five hundred years, though agreement with G. L. Harding's estimate of 1st or 2nd century B.C. is close, namely 1917 ±200 before the present.

On the whole the accuracy of the generally accepted chronology in Egypt is impressively supported by the Middle Kingdom and two of the Old Kingdom figures. The reading for Zoser on the other hand—3979 ±350 before the present—is so obviously too late that one can reasonably suppose some source of contamination. Of wider import to most European archaeologists will be the two prehistoric dates from Egypt; the first from Miss Caton-Thompson's ' Neolithic A ' stage in the Fayoum, and the second from what is described as a ' Middle Predynastic ' context at El Omari in Lower Egypt, 4044 ±205 B.C. and 3205 ±230 B.C. respectively.

The second classification is sufficiently vague, but assuming that no gross error has been made, it will readily be conceded that El Omari should provide a reading appreciably earlier than the First Dynasty, and unless Miss Caton-Thompson's general succession in the Fayoum is seriously at fault, Neolithic A in that locality should be earlier still. Although both these minimum expectations are fulfilled, yet the absolute dates arrived at will come as a shock to many. The very rough dates[2] originally proposed for the two stages in question have for so long been accepted as a basis for discussion, that one has tended to forget that in reality they never rested on more than the most tenuous series of deductions. Probably the most operative of these was the notion that the slow pace of cultural change in dynastic Egypt argued a comparable rate of progress in the earlier periods as well. It may however be pointed out that by no means all the characteristics of the prehistoric communities accord at all easily with this conception.

Such for instance are the far more striking regional differences in a variety of industrial practices, such as pottery and flintwork and the far greater apparent effect of traits of foreign origin. Again is it not perfectly possible that, once the initial discovery of food production had been made, industrial and social development may have proceeded at first at a relatively rapid rate ? It is obvious that two entirely new factors in the situation were introduced at the beginning of dynastic times both in Egypt and Mesopotamia, namely widespread political uniformity and comparative saturation of the economic resources of the country. It needs no great imagination to see in these the main cause of the subsequent slowing up of the processes of cultural change.

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[1] See *Cave Explorations in Iran*, 1949, by C. S. Coon, the University Museum of Pennsylvania, Philadelphia.

[2] Hitherto sometime in the 6th millennium B.C. has been regarded as the most probable date for Neolithic A, while a late 5th or early 4th has been envisaged for the Middle Predynastic.

However this may be the new list of dates certainly makes it seem increasingly likely that the only real hope of enlightenment on equally important topics connected with the ' Neolithic industrial revolution ', such as the relative age of the earliest traces in different areas, and the direction of diffusion of basic practices, lies with an adequate series of carbon analyses for each stage and region. The most interesting of the dates already available outside Egypt, are probably those provided by Jarmo and the Belt Cave. At Jarmo, a stratified occupation mound or *tell* in the Kurdish hills north-east of Mesopotamia, are traces of a village settlement which is believed on typological grounds to represent the earliest stage of such communities so far identified in Western Asia. The carbon date, derived from land snails, is given as 6707±320 before the present. Using double the margin of error this would indicate a period between the middle of the 6th and end of the 5th millennia B.C.

It has however been pointed out, that it is not in *tells* that we may expect to find the traces of the beginning of the Neolithic, since it is clear that the architecture of villages must have been invented later than the technique of food-production which made permanent settlements possible. It is accordingly in repeatedly occupied sites of the type used by hunter-gatherers, such as caves or spring-side camping areas that we may be able to identify the true horizon of the initial discoveries. One such seems to be provided by the newly investigated Belt Cave on the southern shores of the Caspian, referred to above. Unfortunately the assays—dating the earliest signs of the domestication of sheep and goat—are based on undesirably small samples, with a correspondingly large margin of error, namely 8085±1400 before the present. These remains are stratified in a continuous sequence above layers showing unmistakable evidences of a purely hunter-gatherer culture of ' mesolithic ' type.

No dates are yet available for the equivalent stage in Palestine or Syria, or the first village settlements of Mesopotamia, but remains believed to represent the first settled culture on the Anatolian Plateau, namely the ' Chalcolithic ' layer at Alishar, are dated to 4519±250 before the present. Though perhaps not inconceivable this is certainly considerably later than expected, and will require further confirmation from other sites before it can be accepted with confidence.

Second only in interest to the origin of the Neolithic is the problem of the dates of the Upper Palaeolithic in Western Europe. Investigation of this has yet scarcely begun, and two major difficulties have to be faced. First, as already mentioned in the previous article, the maximum period which can be measured with anything approaching confidence as yet is in the order of 20,000 before the present, a date which cannot be far removed from the figures which it is desired to establish. Secondly it is clear that such minute reactions as those yielded by samples of this order of antiquity will be disproportionately affected by a degree of contamination which, at a later stage, would be quite insignificant.

The single figure for the French Upper Palaeolithic is that of 15516±900 before the present, for remains attributable to the latest Gravettian or earliest Magdalenian stages from the famous decorated cave of Lascaux. Opinion is already sharply divided on the significance of this date, which by many is regarded as far too late.[1]

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[1] As we go to press the following dates before the present are released for the Magdalenian III horizon at La Garenne (Neuvy St. Sepulchre, Indre), 11109±480, 12986±560 (both from carbonised bone), and 15847±1200 (ashey matter mixed with sand). (See *Mem. Soc. American Archaeology No. 8*, July 1951; the first date and locality omitted in error).

A possible approach to this question is afforded by the results offered for the various post-glacial climatic stages. Where these are based on peats and muds they seem to be highly variable to say the least—6555±280 for pollen zones II to III in England for instance, whose expected date by an extension of varve chronology would have been approximately 10300. On the other hand a wood sample from Dr Graham Clark's well-known site at Star Carr in Yorkshire, attributed to pollen zone IV (the Pre-Boreal) with an estimated age of about 9800 before the present, yields a carbon date of 9488±350. Carbon dates for the earlier Allerød stage, given as 11044±500 (Hannover, Germany), 11310±720 (Knocknacron, Ireland), 9861±500 (Hawks Tor, Cornwall), and 11442±640 for the correlated stage (Two Creeks) in America, seem to agree substantially with the varve date of approximately 11000±500. The results for the Boreal phase immediately *following* that of Star Carr are given as 7583±380, 9929±350, 8631±540, 9425±540 (all from Denmark) and compare with 7600–8800 estimated from varves.

Earlier Late Glacial figures are few, though it may be recalled that the *varve* dating proposed for the Hamburgian archaeological stage of North Germany runs approximately from 14500–15500 before the present. On archaeological grounds the Hamburgian has been claimed by A. Rust to be closely allied to the later Magdalenian of France, though this is contested by some French authors. If Rust is right, however, it must be admitted that the whole scheme from Lascaux to the later Post-Glacial stages is not without considerable consistency.

One other palaeolithic dating given in the list has been the subject of some comment, namely that of Fontmaure, France, where the date of the Mousterian is given as 973±230. With regard to this site the writer can only state that when he examined the collection in 1949[3], he formed the strong opinion that it consisted of a mixture of Mousterian with specimens of much later character, in all probability mesolithic. The site is not ' a cave ' as described in the catalogue of carbon readings, but an open site in which the finds occur approximately 60 cm. from the surface, and it may be added that the identification of the charcoal in question showed only three species—two of oak, and one of chestnut[4].

The Ponders End deposits in the Lower Thames, correlated with the Late Levalloisian or Mousterian by King and Oakley[5], is the only serious instance of dated deposits equated with this culture stage, and provided a date of ' over 20,000 '.

Of later prehistoric readings in Western Europe the following may be mentioned. A piece of charred wood from Ehenside Tarn suggests a date of 4964±300 (3314–2714 B.C.) for Neolithic ' A ', which seems not wholly inconceivable. Peat attributable to an Iron Age to Roman period from Shapwick, Somerset, and providing a date of 3310±200 (1560–1160 B.C.), however, needs no comment. A sample of beeswax said to have been found with Late Bronze Age specimens, which dated to 819±160 before the present, is also not encouraging for the use of this material.

In conclusion it may be said of the Old World samples so far available, that although some correspond to accepted notions quite nicely, and others are obviously quite wide of the mark, there remains a considerable intermediate category which if correct, would require, to put it mildly, a considerable readjustment of current views. The upshot of the contest will be eagerly awaited by archaeologists in many different fields ; but at least it may be said that results of wide significance should certainly follow as soon as well attested readings are sufficiently numerous to allow of adequate statistical treatment.

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[3] Before there had been any suggestion of taking a carbon sample.

[4] Pradel, *Bull. Soc. Prehist. Française*, 1947, pp. 28-30 and 1945, pp. 84–93.

[5] King and Oakley, *Proc. Prehist. Soc.*, 1936, pp. 66–7.

Notes and News

RINGHAM LOW: THE REDISCOVERY OF A DERBYSHIRE CHAMBERED TOMB[1]

Ringham Low (Monyash) was recorded by Bateman in the mid-19th century as a barrow of considerable size and importance[2] (FIG. 1). He shows plans of the barrow and of four of the chambers which it contained. At that time continuous stone-robbing

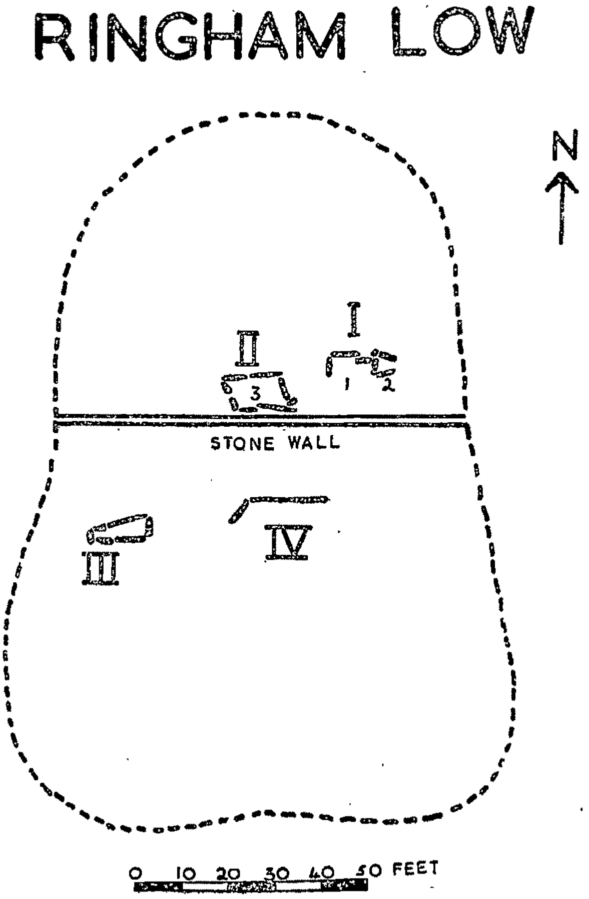

FIG. 1

was taking place, so that, when Ward wrote forty years later[3], the chambers had all been destroyed. Since then, the location of the site itself has been forgotten and it could not be discovered by Phillips[4]. Recently, local enquiries revealed, however, that a few fields lying to the east of Ricklow Dale still bear the name Ringham Low. Within these, about 650 yards to the east of Ricklow Dale and 450 yards to the north of Lathkilldale, the O.S. shows a small plantation with tumuli (MR 170/655), which must clearly be Bateman's site. The barrow lies partly beneath this plantation, partly to the north. To the north of the plantation, the barrow has almost completely disappeared, and no signs of chambers remain. Within the walled plantation, the stony ground is covered with undergrowth and any structures are now indefinite. Confusion has probably arisen as Bateman records Ricklow Dale as lying to the east of the barrow ; in fact the barrow lies to the east of the dale.

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[1] This note is published in anticipation of a general paper on the Peak collective tombs, which has been delayed by the authors' absence abroad. It is hoped that other field-workers will now have an opportunity to give the site the thorough examination which it deserves.

[2] T. Bateman, *Vestiges*, p. 103, and *Ten Years Diggings*, p. 94, et seq.

[3] *V.C.H. Derbyshire*, Vol. 1, J. Ward, 'Early Man', pp. 165–8.

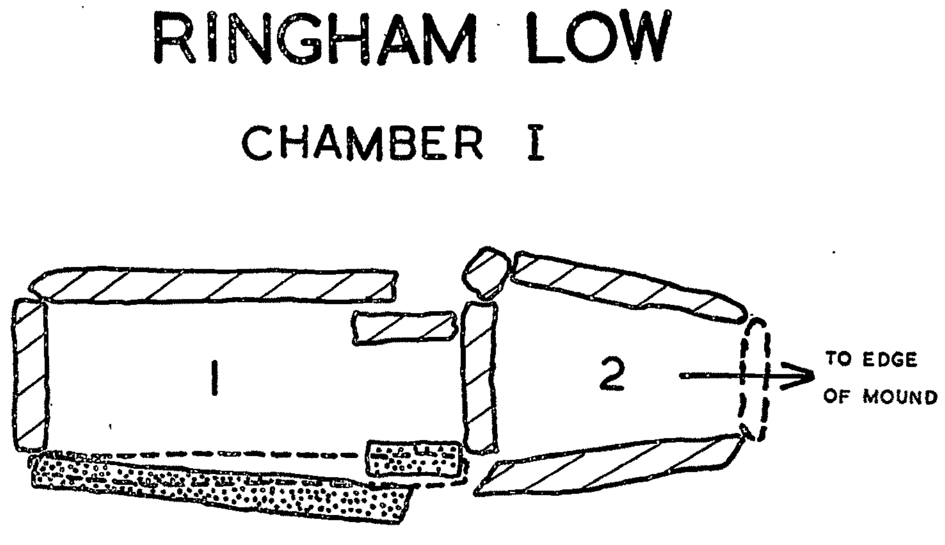

FIG. 2

Daniel[5] dismisses Ringham Low as a group of large cists, but it seems more likely that one at least of the structures is a chambered tomb of the usual Peak type[6]. This chamber (FIG. 2) consists of two cells, containing six skeletons ; it had already been disturbed when Bateman planned it, and the south wall of cell 1 was missing. Bateman attempts to reconstruct the chamber but he gives no evidence for his plan. He makes cell I rectangular and cell 2 wedge-shaped; it would seem more reasonable to reconstruct the south wall of cell I on the analogy of the north wall, where one long slab overlaps a much shorter one. This would give a slightly tapering chamber entered through two pillar-stones closely comparable with the chambers at Minninglow and at Five Wells[7]. Cell 2, which is towards the border of the mound, may be a subsidiary chamber, or, as the authors hope to show in a general discussion on these tombs, the remains of a passage. Bateman does not give the heights of the individual stones, but he does give the depth of each cell; in the case of cell I, 3 feet 6 inches, and of cell 2, only 2 feet. These measurements, which presumably show the average height of the side-walls in each case, would make both cells very low, but they may have been built up with dry-stone walling.

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[4] *Ordnance Map of the Trent Basin* showing distribution of long barrows, megaliths and habitation sites, pp. 10, 17 and 22. It should be noted that the map requires modification as the site is to the east of Monyash.

[5] G. E. Daniel, *The Prehistoric Chamber Tombs of England and Wales*, p. 183.

[6] The type, single-celled, wedge-shaped and entered between two pillar-stones from a passage was discussed by Ward, writing on Five Wells (J. Ward, *Reliq. and Ill. Arch.*, N.S. VII, p. 229–42). Daniel, relying exclusively on plans, has obscured several important features. The whole question will be discussed at length in the paper already referred to. It should be added that the Bridestones has no *morphological* claim to a place in the Peak group.

Two leaf-shaped arrowheads were found in cell I in association with four of the skeletons, and three were also found in the interstices between the paving stones of Chamber II. This chamber contained twelve bodies and is clearly a second collective tomb, but it is not possible to make anything of Bateman's plan and description. There were no other finds of interest in the barrow, but similar arrowheads were also found by Ward at Harborough[8], and are common in megalithic tombs. In short, Chamber I at Ringham Low provides another example of the type of collective tomb characteristic of the Peak, and it may be that careful excavation would reveal the original form of the barrow and its chambers. LESLIE AND ELIZABETH ALCOCK.

ROMAN COINS FROM ICELAND

The discovery of Antoniniani of the emperors Aurelianus, Probus, and Diocletian on Iceland which has been discussed in this Periodical two years ago[1] and which appears to be connected with a find which is beyond suspicion came rather surprising because human settlements on this North Atlantic island are attested by our written sources for as late a time only as five or more centuries after Diocletian. My predecessors in discussing the Iceland coins have tentatively connected this new evidence with Roman Britain, either by attributing the first certain visit of Europeans to Iceland, which appears to be indicated by the three Antoniniani, to Romano-British seafaring merchants or even to a Romano-British warship of the separatist Roman emperor of Britain, Carausius.

Unfortunately these scholars did not look up the mints by which the three coins were issued originally and which can be established with certainty for Antoniniani as a rule. From the most up to date survey of Roman Imperial Coinage which we possess for the later 3rd century A.D. at the moment—that of Mattingly and Sydenham[2]—the following information about the well known and fairly common types of the Iceland coins has to be added to the earlier detailed descriptions of the specimens by Shetelig and Eldjärn:—

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[7] Most easily accessible in Daniel, op. cit., p. 85, fig. 24.

[8] J. Ward, *D.A.J.*, 1890, Vol. XII, p. 118, et seq.

[1] Cp. with earlier bibliography, H. Shetelig, 'Roman Coins Found in Iceland', ANTIQUITY XXIII (1949), 161 f.; K. Eldjärn, 'Fund of Romerska Monter Pa Island', *Nordisk Numismatisk Årsskrift* (1949), 1 f. I have to thank my pupils, K. W. Stubington for having translated Eldjärn's rather instructive article into English, and especially F. H. Armstrong, B.A., Assistant in the Royal Ontario Museum of Archaeology who was most helpful in checking Eldjärn's coin descriptions with the relevant specialist numismatic literature.

[2] Cp. for the numismatic problems discussed in this article H. Mattingly-E. A. Sydenham, '*The Roman Imperial Coinage*', vol. V, 1 (1927); V, 2 (1933); K. Pink, 'Probus', *Numismatische Zeitschrift*, 73 (1949), 13-74.

1. Antoninianus of Aurelianus. Mint of Cyzicus. Cp. Matt.-Syd., *op. cit.* v, 1, p. 306, no. 361.
2. Antoninianus of Probus. Mint of Rome. Corr. on Rev. RA to R△. Cp. Matt.-Syd., *op. cit.* v, 2, p. 37, no. 187.
3. Antoninianus of Diocletian. Mint of Rome. Cp. Matt.-Syd., *op. cit.* v, 2, p. 236, no. 161.

It appears from the foregoing that the two younger Antoniniani were minted in the city of Rome itself with certainty. On the other hand no. 1 seems to have been issued at Cyzicus on the Asiatic side of the Propontis, a mint which was of importance both for Asia Minor and the Balkan peninsula. A careful inspection of the originals in Iceland by an experienced numismatist may still lead to slight corrections in their attribution to catalogue varieties. Nevertheless already now it is obvious that none of the three Antoniniani was minted in Britain, Gaul, or Spain. Under these circumstances it is most unlikely that they were, as has been suggested in earlier articles, the property of a Romano-British or Western provincial merchant who had acquired them by buying and selling some merchandise in the western part of the Roman Empire with its numerous imperial mints.

Similarly the naval ratings of the fleet of the rebellious emperor Carausius who appear as *dei ex machina* in Eldjärn's theories about the last owners of the coins were certainly not paid with Antoniniani from mints which were controlled by the inimical emperors Diocletian and Maximianus Herculeus. Instead it looks in my opinion as if the owner or the owners of our three coins had lived in Asia Minor or near the Danubian frontier of the Roman Empire originally, but took residence in Rome during the years between the later reign of Aurelianus and the earlier years of Probus, and left the eternal city and the territory of the Imperium Romanum from unknown reasons for adventures on the Atlantic Ocean under Diocletian.

Most attractive is, under these circumstances, to presume that the owner or owners of the three coins were first members of the Roman field armies on the Danube or in the Orient and later of the garrison of Rome, all three military establishments being strongly recruited from free Germany near A.D. 300, the approximate date of our find[3]. Perhaps the three coins were left on Iceland by members of the crew of a Scandinavian or Saxon ship who had served in the Roman army in earlier time, but were now raiders, pirates and adventurers on the Atlantic with the result that their ship was driven by a storm close to the still undiscovered American continent. To bury votive gifts in times of distress, as was presumably done with the three Iceland Antoniniani, is a well known Germanic custom which leads back to the Neolithicum of Northern Europe[4]. The new coin find from Iceland is therefore best interpreted, in my opinion, as evidence for early Germanic visitors to the remote island.

The much discussed Thule problem has to be reconsidered in the light of the new evidence also. The island of Thule of which the Greek merchant adventurer Pytheas of Massilia was told in Britain near 320 B.C. has been considered to have been Iceland by many serious scholars for a long time[5]. The main obstacle for such an interpretation of Pytheas's report was that honey from bees was supposed to have been obtainable on this Thule, Iceland being too far to the north for the varieties of *apis mellifica* to survive. But recently the plausible suggestion has been made that Pytheas's honey was collected, not from domesticated, but from the hardier wild bees which indeed belong to the fauna of Iceland[6].

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[3] Cp. with earlier bibliography A. Schenk von Stauffenberg, *Das Imperium und die Völkerwanderung* (1947), 7 f., 212 f.

[4] Cp. K. Helm, *Altgermanische Religionsgeschichte*, 1 (1913), 243 f.

[5] Cp. with full bibliography M. Cary-E. Warmington, *The Ancient Explorers* (1929), 33 f., 36 f.; J. O. Thomson, *History of Ancient Geography* (1948), Index *s.v.* Pytheas, Thule; Pauly-Wissowa, ' Realenzyklopädie der klassischen Altertumswissenschaft ', art. Thule; *Oxford Classical Dictionary*, art. Pytheas, Thule.

Furthermore if Iceland could be reached by ships from Europe in A.D. 300 or so as is now practically certain, then there is no reason to doubt that the island was approached occasionally by the already similarly well-built vessels of the Hellenistic Age also. The discovery of the three Antoniniani makes it therefore as certain as the general state of our scanty and all too derivative written sources permits that Pytheas of Massilia and the rather sceptical Graeco-Roman readers of his published reports during the Hellenistic and Roman centuries actually had some information about Iceland at their disposal. Pytheas, this extraordinary seafarer of Marseilles, might have discovered America long before the Vikings and Columbus if he had continued his journey in a westerly direction.

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[6] M. Cary in *Classical Review*, 63 (1949), 112.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

DREVNAYA ISTORIYA YUZHNOY SIBIRI, by S. V. KISELEV. No. 9 of the Series which started as *Matériaux et Recherches d'Archéologie de L'U.R.S.S.* but has now dropped its French title (!) and bears date Moscow, Leningrad, 1949, 4to, 364 pages, 62 plates, many maps. [A fine coordination of the history and archaeology of Southern Siberia from neolithic to Mongol times. French resumé by R. Ghirshman in Salmony's *Artibus Asiae* XIV, 1951, 169–89].

BANDKERAMISCHE STUDIEN, by AUGUST STIEREN and EDWARD SANGMEISTER : 33 Bericht der R.–G. Kommission. [The authors adduce evidence to justify the thesis that the earliest cultivators in Central Europe (the Danubians) lived not in irregular pit-dwellings but in very long houses; and they draw highly plausible inferences about the organization, population and duration of these settlements].

ENGLISH SOCIETY IN THE EARLY MIDDLE AGES, by DORIS MARY STENTON. *Pelican*, 2*s* 6*d*. [A description, drawn largely from contemporary sources, of England in the 2½ centuries after the Norman Conquest, its government, the life of rich and poor, its economy and its Church affairs. A book by Lady Stenton does not need our commendation ; it is a wonderful book for so small a price—the harvest of a life-time's work, cast in a popular mould].

KHARGA OASIS IN PREHISTORY, by G. CATON-THOMPSON, with a Physiographic Introduction by E. W. GARDNER. Univ. of London ; Constable & Co., 12 Orange St., London, W.C. 2, 1952.

PREHISTORIC EUROPE : the Economic Basis, by J. G. D. CLARK. 350 pages, 16 plates, 180 text illus. Methuen & Co., 1952.

LES VOIES ROMAINES DE L'AFRIQUE DU NORD, by PIERRE SALAMA. Gouvernement General de l'Algerie : Service des Antiquités. Algiers, 1951 : 143 pages, 12 half-tone plates, map (in colour) 1 : 500,000. [We have not seen this book because the publishers, who have probably not yet discovered ANTIQUITY, have not sent us a copy ; but we are told that it is an ' exhaustive survey with gazetteer of Roman sites, and very useful, as the corresponding sheets of the Tabula Romana are not yet published '].

Reviews

MAP OF MONASTIC BRITAIN: Scale 1 : 625,000: *published by the Ordnance Survey*, 1950, *two sheets.* *7s 6d per sheet.*

This latest addition to the series of National Period Maps published by the Ordnance Survey has been compiled by Mr R. Neville Hadcock. The two sheets shew the geographical distribution and the character of the monastic houses of Great Britain between the Norman Conquest and the Dissolution of the Monasteries. Each sheet is prefaced by a short introduction, which lists the authorities consulted, and a combined index to the two sheets.

For the initial date 1066, has been chosen so that the map gives a full picture of the new and reformed orders, together with the Benedictine houses which had been founded at an earlier date. In this sphere Mr Hadcock's work is thorough and exhaustive. The Director General in his foreword appeals for corrections by students of local history; some will surely be forthcoming, but they are likely to be few and of little importance. A random examination in several districts has shewn that the lists and classifications need little revision. Two minor corrections may be noted. There is some evidence that Soulseat, correctly shewn as a Premonstratensian house, was first founded for the Cistercians. In Devon Harberton and Bothemescombe, indexed as alternatives, are separate sites. The latter lies in the parish of Uffculme (now Bodmiscombe) and seems to have been the principal holding of the Hospitallers in Devon.

In Scotland and Wales the date 1066 is less satisfactory. Older customs survived in these areas to a much later date and the attempt to shew these survivals on the map is neither consistent nor comprehensive. It would have been simpler to adopt the rigid medieval theory, which did not regard the *clas* or its equivalent as a monastery in the full sense of the word. The result would have been the deletion of many signs that confuse the main issue and do not really add to our appreciation. A few examples must suffice. In the north the life and writings of St. Ailred shew that communities of priests or monks survived into the 12th century at Kirkcudbright and Whithorn; neither is indicated on the map alongside the later reformed Orders. In Wales, Holyhead is shewn as a collegiate church, while Clynnogfawr in addition to the same symbol has a second for the extinct *clas*; yet these establishments have a similar history, the primitive *clas* becoming a collegiate church in the course of the Middle Ages. The choice of sites to be marked with the symbol of a *clas* is also open to question. Churches, like Corwen and Llansannan, which were portionary in 1291 do not appear on the map, though they have a better claim than an ancient church like Llandrillo, which had certainly ceased to be a *clas* by the early 13th century. There are similar anomalies in Cornwall, where Bodmin, though marked as Augustinian house does not receive the symbol of an extinct *clas*.

A kindred point arises in the text about the Scottish sees, a paragraph which is too condensed to cover a very complex subject. The omission of Aberdeen is clearly an oversight, but Galloway was not revived till *c.* 1125. There is evidence, admittedly of uncertain value, that both Orkney and Aberdeen have a succession reaching back before 1066. Furthermore the classification together of the ' additional sees ' tends to destroy the contrast between new creations such as Caithness and Moray, which cover unified areas, and sees like Brechin and Dunkeld, which are composed of scattered districts and can be most readily explained as the survival of Celtic bishoprics of the older, jurisdictional, non-territorial type.

These criticisms refer to a minor and non-essential feature of the map. They do not detract from its very real value as a picture of an essential part of the ecclesiastical life of the Middle Ages. The symbols are clear and well chosen and the general appearance and execution of the sheets fully conforms to the very high standard, which Ordnance Survey has set itself in this series. C.A.R.R.

MISCELLANEA MUSICOLOGICA (I–II), 1948. 25 *pages*, 2 *plates*, 22 *figures*.
MISCELLANEA MUSICOLOGICA (III–VI), 1949. 31 *pages*, 7 *plates*, 12 *figures*.
CYMBALES ET CROTALES DANS L'ÉGYPTE ANCIENNE, 1949. 95 *pages*, 10 *plates*, 51 *figures*.
CATALOGUE GENERAL DES ANTIQUITÉS ÉGYPTIENNES DU MUSÉE DU CAIRE, Nos. 69201–69852, INSTRUMENTS DE MUSIQUE, 1949. iv—216 *pp.* 116 *plates*, 74 *figures*.
All by DR HANS HICKMANN: *printed in Cairo by Imprimerie de l'Institut Français d'Archéologie Oriental for the Services des Antiquités de l'Égypte.*
LA CLIQUETTE, UN INSTRUMENT DE PERCUSSION ÉGYPTIEN DE L'EPOQUE COPTE. *By* DR HANS HICKMANN, *Cairo, Imprimerie de l'Institut Français d'Archéologie Oriental* 1950 *for Société d'Archéologie Copte.* 12 *pp*, 8 *figs. No prices given.*

Of the Miscellanea, nos. I, II, V, VI, describe the structure and methods of tuning of Ancient Egyptian harps, lutes and lyres, incidentally making it clear that tuning by movable pegs is a comparatively modern innovation. Pegs were used merely as anchors for the strings which were tuned by being wound round the necks of the instruments, or unwound. The harp is the most characteristic instrument of Ancient Egypt, but Dr Hickmann shews that it is the lute in its various forms which is the ancestor of most modern stringed instruments. No. IV of the *Miscellanea* argues that certain perforated shells found in prehistoric burials were used as whistles.

No. III of the *Miscellanea* is an extremely interesting description of the way in which a contemporary Coptic church singer accompanies his singing with movements of his hands (chironomy) to remind choirs or pupils of the rhythm and shape of the phrases they are singing or learning. The singing is alleged to be in Ancient Egyptian and Early Christian style, and the author deduces that persons depicted by the Ancient Egyptians as appearing to conduct other musicians are in fact chironomists. The various forms of musical notation derive from chironomy. This surely is a performance which should be recorded on a sound film before the technique disappears.

The Monograph on Cymbals and Crotales (small bells) is devoted mostly to Cymbals, and describes all shapes and sizes. These instruments date from the New Kingdom, Greco-Roman and Coptic periods only, and were imported from Asia, having originated from ritual cups and censers.

The Catalogue appears to be a very complete one, and includes sound-making objects and bell moulds. The instruments are classified in groups, and appendices giving cross references which make it simple to trace any item. Every type of instrument is fully illustrated.

Many examples of the Cliquette or Clappers survive, but mostly in pieces so that they have not normally been recognized as musical instruments. They are in fact similar to handled Castanets. The author shews them to have been used in Coptic church services, and to have regulated Monastic life as the precursors of bells during the 3rd to 6th centuries. P. J. M. DAVIES.

REVIEWS

MONMOUTHSHIRE HOUSES : PART I, MEDIEVAL HOUSES. *By* SIR CYRIL FOX *and* LORD RAGLAN. *The National Museum of Wales, Cardiff,* 1951. *pp.* 114, 20 *plates,* 54 *text figures and* 4 *sketch maps. Price,* 17*s* 6*d.*

The scientific study of peasant building in the British Isles has long been the Cinderella of our folk culture. Addy in 1910, *The Evolution of the English House*, and Innocent in 1916, *The Development of English Building Construction*, made excellent pioneer studies and since then there has been a steadily growing literature, much of it in the journals of local societies. In the first part of their work on Monmouthshire houses, which covers the period from 1415 to 1560, Fox and Raglan have produced a thorough survey of the small houses of the rural hinterland of that county.

Cruck-framed buildings provide one of the most interesting of our early types and both Addy and Innocent dealt fairly fully with this structure. Innocent defined its southern limit as a line drawn from the Wash to the Bristol Channel but since then the assiduous collecting of N. Teulon Porter (unpublished material in the possession of the reviewer) has extended the boundary far to the south and west of this line. The earliest dated English example is the Enstone tithe barn, which was built in 1382, but there is a record of ' two bent beams called " crokkes " ' at Harlech in 1278 and Henblas, Llandderfel, Merioneth, is considered to be early 14th century. On the Continent, Klein has described a cruck building from Westphalia dating from A.D. 350 and the work of Trefois, van Giffen and others has established cruck distribution extending in a band some 45 miles wide from West Flanders to Denmark.

The major part of the book under review is devoted to a description of thirty-seven cruck-framed buildings in Monmouthshire. The earliest example, that of Pistyll, Tegare, is placed between 1420 and 1450. It is a clumsy truss with wide feet, a single low collar beam and no tie beam. Subsequent development resulted in a characteristic paring of the cruck foot and either the use of the tie beam to support the wall-plates or the more interesting innovation of slotting heavy oak spurs, notched and shouldered into the crucks, to carry the wall-plates. The peculiar reduction of the cruck feet is ascribed to the intrusion of the heavy timber-frame house-building tradition which replaced the earlier light wattle and daub. Ultimately the cruck truss was reduced to an ' upper cruck ' with a much more angular bend and a shorter upright portion which was tenoned at its foot into the ceiling beam. The authors have established this development thoroughly and scientifically but their work is not confined to the cruck framework alone ; it includes an equally detailed treatment of the house-plan and all the attendant features of wall structure, doorways, windows, fireplaces and staircases.

The second part of the book deals more briefly with small medieval stone houses and in Monmouthshire these have a central house-body, separated by a passage at one end from the kitchen and with a solar at the opposite end. The peculiarity of these houses lies in the siting of the house-body fireplace in the centre of the passage wall, a feature common to the 15th century Pennine houses of the same plan. Fox and Raglan suggest that this type is more common, if not peculiar to, the highland zone and they emphasize the difference between this arrangement and the normal ' screens passage ' where one end of the hall is partitioned off by wooden screens and the fireplace is sited in the side wall. It is unfortunate that they have used the term ' screens passage ' for the passages in the Monmouthshire houses as these were probably not the result of simply screening off the entrance end of the hall. The association of a pair of opposite doors with the house-body fireplace is of some significance and indicates that this type of passage probably originated as a means of controlling the fire as I have already suggested for the Pennine houses (*Country Life*, Dec. 3, 1948). The use of a pair of opposite doors for this purpose has been noted by Campbell in Ireland (*Folk-liv*, 1937), by Erixon in several parts of Western Europe (*Folk-liv*, 1937) and by Peate in the Welsh long-house (ANTIQUITY, 1936). Fox and Raglan themselves suggest the possibility of such passages having resulted from the assimilation of the hall-house-with-spere-truss and the long-house plans, although definite proof for this is still lacking.

It is a book full of sound scientific reasoning and one which makes the reader eagerly await the publication of the next two parts. If there is any criticism to be made it concerns the plates. Many of the photographs, such as IIc, IId, Va and VIIIc, do not appear to have been taken with any specific object in view and do not afford even a good general impression of the subject. Sir Cyril Fox is a very good draughtsman indeed and many of the photographs of details, such as the door frame of Upper Trerhiw (Plate XIIb) and the solar window of Blaengavenny (Plate XIVb) would have been much clearer and far more useful if depicted as line drawings. But these are very minor criticisms of a fine book and a valuable contribution to our study of British folk architecture. Certainly no such criticism can be levied against Sir Cyril Fox's line drawings which are a model of clearly expressed architectural information. The National Museum of Wales has done a great service to students of folk culture by the publication of such a regional survey which will serve as an admirable guide for similar studies in other parts of the country. JAMES WALTON.

METALLURGY IN ANTIQUITY. *By* R. J. FORBES. 489 *pages*, 98 *figures including maps and diagrams. Leiden (E. J. Brill)*, 1950. *Price Gld.* 19 (=*about* £1 16*s* 5*d*).

A reliable book on metallurgy from the earliest times has long been a desideratum, and Mr R. J. Forbes has made a gallant effort to supply one. For several reasons he cannot be said to have filled the gap, the chief being that, as he points out in his last chapter, a great deal of research work still remains to be done. Modern methods can tell us a great deal about the origin and the method of manufacture of metal objects, and they are now usually applied in every important excavation; but 'what we need most is a thorough investigation of all the metal remains now buried in our museum store-rooms (p. 477)'. It is true that even microchemical and spectrographic methods necessarily destroy some of the original material, but the amounts are minute. Metallographic examination by grinding down till metal unaltered by surface changes due to conditions of burial, etc., leaves a small area looking much as it did when the object was in use; but surely that is not a serious objection, even if a 'beautiful patina' is thereby destroyed. One great virtue in Forbes's book is the indication of what might be accomplished by collaboration and organisation. What an immense amount of knowledge might be gained if the equipment and the staff of Dr Plenderleith's laboratory could be multiplied by ten!

For practical purposes, however, the effect of a valuable book depends, not only on the amount of information it contains, but also on the number of people who buy it and read it. Unfortunately 'readable' is not a word that can be applied to 'Metallurgy in Antiquity'. One would not judge harshly the English syntax, grammar or punctuation in a book produced in Leiden, even though the publisher is E. J. Brill; but much more care and time should have been devoted to proof-reading: on p. 91 we are told that 'he (Hephaistos=Ptah) was said . . . to have reigned Egypt and some authors like Johannes Malalas and Johannes Antiochenus even contend that he learnt the Egyptians to forge iron weapons'. That sentence illustrates several points: the marvellous bibliographical learning of the author and a serious lack present throughout his book. We willingly accept quotations from unusual authors, but they have little value for us unless the approximate date is given. I must confess that I had never before heard of either of the above authors; but from the *Encyclopaedia Britannica* I learn that I can see a text of the 6th century Byzantine John Malalas in Migne, *Pat. Graec.* xcvii, of John Antiochenus I learn—nothing. Comparatively well-known classical authors are mentioned in an un-English manner—we do not speak of Cassiodor or Diodor.

For most of those interested in the development of metallurgy it would be a real help if approximate dates were given for most of the authors quoted. A warning is rightly inserted in the introductory note that early chronology is often still uncertain, e.g. that the reign of Hammurabi has recently been brought a century and a half later than used to be accepted, but many readers will often be puzzled even after consulting the bibliographies to each chapter: Agricola's *Bermannus* is rightly given as cf 1546, but the much more important and better known *De Re Metallica* appears as Berlin, 1928; the original date, 1556, is essential to the relevance, and a book in English should give the Hoover translation (London, 1912) rather than a German edition. *Biringuccio* is once (p. 373) entered as 'Venice, 1540', but usually only under a German edition as 1925 (no doubt the American translation is too recent for inclusion). Theophilus *De Diversis Artibus* is often quoted, but is listed only under the name of Theobald, a German editor with date 1933; he should appear as xi cent. A.D. (English translation, 1847).

On many subjects antiquaries nowadays are apt to neglect the evidence of the Hebrew literature, perhaps because 'the Bible is Theology'. Forbes, on the contrary quotes every available allusion from the Old Testament, but again with no dates. The well-known description of metal-mining in 'Job' is, of course, made use of; but the addition of such words as '(now regarded as of about 450 B.C.)' would be an improvement. Some students of early metallurgy might have nothing to refer to but Ussher's dates in the A.V. and think that the passage applied to a date before 1000 B.C.

Despite these criticisms the detailed bibliographies, rightly used, are a most valuable feature of the book; they include many important papers, in several languages, dealing with every branch of the subject.

The first five chapters are general surveys of the subjects involved, metallurgy, mining tools and methods, including an excellent discussion on fuels (pp. 105–112) and a remarkable account in chap. iv of 'the evolution of the Smith, his social and sacred status', literally from China to Peru. The greater part of the book is divided into chapters for each metal or group of associated metals. Gold of course comes first, followed by Silver and Lead; Tin, Antimony and Arsenic; Zinc and Brass; Copper; and finally Iron. For Gold and for Copper the words 'In the Ancient Near East' form part of the chapter title. In fact throughout the book the great emphasis is on the Near East, justifiable perhaps since the great discoveries in metallurgy in the strict sense can nearly all be proved to have arisen there, but somewhat disappointing to the English reader. One might well expect more attention and fuller references to the important Roman works in Britain and to the inferences that can be safely drawn concerning those of the Bronze and Iron Ages. For each chapter there is a useful map showing known sources of ores and, by arrows, the probable directions for imports and exports; but the western margins barely touch the mainland of Greece and the northern fail to cover the Black Sea: for tin, an arrow indicates in the Levant possible imports from 'Spain, Bretagne, Cornwall'. Could we not have been given a map showing these mining areas and the proved tin routes?

On Zinc, Forbes quotes Diergart that the passage in Strabo on 'pseudargyron, which added to copper makes oreichalkos' is 'technical nonsense', and denies any reference to metallic zinc. Apparently he has missed the important discussion on the question in Walter Leaf's *Strabo on the Troad.* Leaf set a splendid example to all commentators on classical literature: he consulted technical experts on the processes employed and Turkish mineralogists on the availability of ores. The only ' nonsense ' is a single word—the preliminary calcining of the ore does not produce *iron*, but zinc oxide. But this substance may have been colloquially called iron by the smelters, or Strabo may have assimilated a technical word strange to him to a familiar one: on p. 77 Forbes himself mentions a ' tribe of iron-smiths . . . who call copper " red-iron ", brass " yellow-iron " '. The present writer prefers to agree with Leaf and with Oliver Davies that metallic zinc was known in antiquity.

Chapter x on Copper is rightly the longest in the book, though confined, as already mentioned, to the Near East. Even so, the bibliography runs to 128 entries; and a vast amount of useful information has been worked into the text. Although accounts of the working of copper in Britain are excluded, recent papers by Smythe, Whittick and other English authors deserve mention for the light thrown on ancient copper alloys.

The chapter on Iron is incondite and difficult to follow. The suggestion that the earliest iron of the Near East, sometimes referred to as ' sporadic iron ' was ' a by-product of the refining of gold ' (p. 403) is interesting and will be new to most readers. On the vexed question of Roman cast-iron it is impossible to acquit Forbes of carelessness involving injustice to English workers. He has apparently not seen T. May's important work on *Warrington's Roman Remains* (1904), but mentions only a reference to it in Wyndham Hulme's review of Rickard's *Man and Metals* in ANTIQUITY, vol. VII. The Roman cast-iron found at Tiddington near Stratford-on-Avon, published, with convincing micro-photographs, by Fieldhouse, May, and Wellsford in 1931 is ignored, though the same review in ANTIQUITY mentions it, and concludes that ' it is clear that the Roman metallurgists could produce cast-iron and were acquainted with its properties ', a verdict with which I wholly agree. Despite this blemish there is a vast amount of accurate information scattered through the chapter: a summary on p. 414 (the chapter ends on p. 467) should be digested by all archaeologists:—' Technically speaking we must, therefore distinguish the following phases of iron industry. First a period of the use of meteoric iron, then iron produced as a by-product of gold-refining, followed quickly by the evolution of the reduction of iron ores in bloomery and shaft-furnaces, which rose to prominence when methods of cementation allowed the manufacture of steel. For only steel is definitely better than bronze for tools and weapons and only the invention of steel could herald the Iron Age '.

It is because *Metallurgy in Antiquity* is a unique and indispensable work of reference that we have thought it worth while to criticise so many minor points. Is it too much to hope that Forbes himself will be able to revise it ? C. N. BROMEHEAD.

ARREST AND MOVEMENT. *By* H. A. GROENEWEGEN-FRANKFORT. *Messrs Faber and Faber Ltd., Russell Square, London, W.C.* 1, 1951. *pp.* 222, *pls.* 96, *and many line drawings. Price* 50*s.*

Mrs Frankfort has written an interesting and challenging book which bears evidence of much careful research and intensive thought. Her training in philosophy leads her to view her subject from an unusual angle and, if the phraseology she employs makes it somewhat difficult for the ordinary reader to comprehend her meaning at times, this should not deter him from appreciating her enlightening interpretation of many aspects of art in the Near East.

We feel in studying the artistic products of these peoples, so remote from our own world, that we are in danger of attributing a modern viewpoint in appraising their merits or demerits. Mrs Frankfort is aware of this when she says (p. 11) ' the interpreter is in constant danger of reading into a scene what he wants to deduce from it and of explaining the absence or presence of features which do not tally with his interpretation as artistic incompetence '. That she does not always avoid this danger, which of course is very difficult for a modern critic to do, is not surprising.

We must not forget that what we term ' artistic ' or ' beautiful ' had very little meaning for the people she writes about, or for any other primitive community. Their paintings and sculptures served definite purposes and their beauty, when we feel it in particular instances, was only incidental and unconscious. Nevertheless individual artist-craftsmen show a superiority above their fellows and thus appear to be possessed of a higher sense of beauty than the rest.

The scenes at Beni Hasan are described as ' very dull ' but allowance should be made for the inadequate reproductions of the Egypt Exploration Society's volumes. These are on so small a scale that most of the interesting details are lost. The starveling herdsman illustrated on pl. xxva has his counterpart in paint at Beni Hasan[1]. It is true that much is crude and provincial in the paintings (which are obviously by different hands), but the artists who painted the birds in the mimosa bush[2] as well as many other birds and animals show a sense of form and colour which is quite extraordinary in any period.

The statement on p. 78 that Antefoker is the only remaining Middle Kingdom tomb at Thebes must be modified. The tombs of Daga[3] and Djar[4], both of that period, contain a number of extant scenes.

Her interpretations of the scenes on the walls of New Kingdom tombs sometimes invite queries. Granted that the deceased in Old Kingdom representations is shown merely ' watching ' (p. 31) activities as a kind of passive ghost, this hardly seems to apply to many episodes in the New Kingdom. Shooting at game or fishing and fowling by the owner of the tomb may or may not have been definite events in time ; with the Egyptian tendency to exaggeration many more animals and birds than he could have hoped to see together in his lifetime are provided, but he is evidently taking an active and joyful part in hunting them. The pastime is reminiscent of his days on earth although the magical element may not have been altogether absent[5]. There are however instances where such scenes are purely imaginary—a kind of wishful thinking. Tutankamūn certainly did not fight Nubians or Syrians nor slaughter so many lions as he is shown doing on his casket. But possibly this was flattery on the part of the artists ; we are very familiar with the exaggeration of the king's prowess everywhere in the New Kingdom.

It has to be borne in mind that the tombs were begun during the life-time of the owner and he had the superintending of the pictures arranged on the walls. These scenes in which he was personally interested would naturally be depicted there and have a biographical meaning. When he died his tomb, in many instances, was left unfinished and it is interesting that the walls on which funerary scenes are generally pictured are those which are often left blank. The heirs, apparently, did not trouble to add the necessary burial rites closely connecting the deceased with the Hereafter.

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[1] *M M.A. Bull*, Sect. II, 1931–2, Figs. 9, 10.

[2] *Ancient Egyptian Paintings*, pl. IX.

[3] Davies, *Five Theban Tombs*, pls. XXIX–XXXVIII.

[4] Winlock, *Excavations at Deir el Bahri*, pl. 17.

[5] See Sir Alan Gardiner's remarks on this subject in *The Tomb of Amenemhēt*, pp. 14–20, pp. 38–9.

We must not forget how much has perished in the tombs and what is lost (guessed at by traces), often belonged to scenes of more interest than the commonplace survivals. Not only are iconoclasts responsible for this but contemporary enemies of the tomb-owners. For example no full-faced musicians have survived in the tombs owing to the prejudice of modern Egyptians against the evil eye: had the picture in the British Museum (pl. XXXIVa), not been taken away from the necropolis soon after it was discovered that too would have been mutilated. The full-face was not so rarely pictured; at least one other 18th Dynasty example (in tomb 249), can be added to those on Wreszinski's list.

Scenes said to be ' unique ' have a tantalizing habit of reappearing and it is perhaps rash to say that the garden in Sebekhotep (p. 90) is the only one of its kind when parts of similar designs can be traced elsewhere.

Personal and actual experiences do not seem so unusual as Mrs Frankfort suggests. The scene she cites on p. 80 where Amenemhet is fighting a hyena is surely to be attributed to him since his title *(hry) n pḏt*, is inscribed above the beast[6] and is found twice elsewhere in connection with him[7]; so there seems no reason to doubt he is identified with the action. The receiving of the Puntites by the owner of tomb 143[8] and the landing of the Syrians from their ships to barter with the Egyptians in tomb 162[9], are perhaps only a few of such occurrences in which the biographical element, called in question on pp. 79–80 is present.

On pp. 73, 84–5, some interesting comparisons are drawn between hunting scenes in Egypt and Assyria illustrated by Figs. 11, 14, and pls. LXXIXa, LXXIXb. The former are composed with the help of ground-lines and the latter without them.

Perhaps the reason why battle scenes are not found in private tombs of the New Kingdom (as noted on p. 88), is that, since the king is always the dominating figure in them, it would be usurping the glory of the Pharaoh vanquishing his foes to record them elsewhere than on temple walls (which were his prerogative), or on his personal objects. The private person had a very subordinate part in his pictured victories.

In the appraisement of Egyptian art of this period the freely-rendered and unconventional ink drawings on ostraca and on walls should not be ignored. Madame Vandier-d'Abbadie has excellently reproduced many of these[10]. Others, such as those found by Mr Winlock[11], are illuminating as to what the Egyptians could do when untrammelled by convention.

Turning to the chapters on Mesopotamia, one feels the early sculptures, especially those on pls. LVI, LVIII and LIX, are almost repellent in their ugliness. They unfortunately remind one of the unhappy products of some artists of our own time!

It is very different when we come to Assyrian art from about 800 B.C., when Egypt was already decadent. There low-relief sculpture attained a level of superb excellence and skill; characterization of form and movement of wild animals could scarcely be better done. Mrs Frankfort knows so much about Mesopotamian culture that her comparisons with Egyptian art are of great interest. It might have been stressed that the introduction of the horse and chariot in the time of the Hyksos was responsible for the appearance of fresh elements in Egyptian compositions from the reign of Tuthmosis III onwards.

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[6] *J.E.A.*, vol. XXVI, 1940.

[7] Sethe, *Urk*, 906aa, 900i.

[8] *M.M.A. Bull.*, Sect. II, 1934–5, pp. 46–48.

[9] *J.E.A.*, vol. XXXIII, 1947, pl. VIII.

[10] J. Vandier d'Abbadie, *Catalogue des Ostraca figurés de Deir el Medineh.*

[11] H. E. Winlock, *Excavations at Deir el Bahri*, pl. 92.

The section on Cretan art which ends the book, is perhaps the most suggestive and illuminating. It is headed the ' Cretan Enigma ' and, in truth, can be termed nothing else. Its origin and its end are both obscure and in its manifestations we are aware of an inherent decadence which is baffling. Its isolation from Egyptian and Near Eastern cultures is well illustrated in those pages. On p. 186 it is justly noted that in contrast with Egyptian and Mesopotamian art, Cretan artists ' avoid depicting human achievement whether in the modest sense of manual work ' or ' in the challenging one of battles won '. It might be added that the absence of any representations of ships is strange in an island community which had relations with the mainland. It is emphatically true that the feeling for beauty of form and design and the translating of material objects into lovely patterns has seldom been so satisfactorily attained. On p. 189 ' ingenious reconstructions which are sometimes unsound ' are referred to in connection with the paintings: Too much imaginative restoration is very apt to lead astray.

In conclusion the author must be congratulated on the fine photographic plates. Some of the line drawings in the text unavoidably suffer from the greater reduction and printing on rough paper. Only in one instance is there a mis-print in their numbering; Fig. 44 on p. 212 should read Fig. 45. A.M.D.

A PICTURE-BOOK OF ANCIENT BRITISH ART. *By* STUART PIGGOTT *and* GLYN E. DANIEL. *Cambridge*, 1951. 12*s* 6*d*.

These seventy-three photographic reproductions are good value for the price asked, and it was a good idea to supplement the moderately priced (and excellent) ' introductory books on the prehistory of the British Isles ' by one containing pictures only. So far as art is concerned this book achieves its purpose; the introductory essay of eleven pages gives just so much background information as the intelligent and not too ignorant reader needs, including a short summary table of absolute chronology. The explanatory list of photographs provides all the essential known facts, and, greatly daring, a date in years for each object.

A few minor observations may be recorded. The references to published accounts of the objects would have been more convenient if combined with the plate-descriptions and not printed as a separate list. As it is, one has to turn to two separate places for each illustration. Of course the proper place for both is opposite the plate itself, but one knows that that is impossible nowadays. Speaking from memory we believe the Rillaton gold cup was found in a burial-chamber, not a cist; the barrow is normal and still existed a few years ago when we visited it, and probably exists still. (It would be worth while clearing* and planning it).

Most of the earlier objects illustrated please chiefly by reason of their good craftsmanship, but from No. 37 onwards, we are well within the realm of art, and things like the Battersea shield and Birdlip mirror are really beautiful. All the latter exemplify the saying that ' genuine art is always produced for current use '. For those who are troubled by doubts about modern art we would suggest another picture-book devoted to mugs, tankards, coins, helmets and personal ornaments, carried rapidly through the Christian era and culminating in those produced for current use. On this basis the Rillaton cup, the Trawsvynydd tankard, the Ardagh chalice and a modern water-jug of moulded glass (borrowed from any restaurant) are all strictly comparable. But a similar series of coins—British, Saxon, medieval and modern Irish—would set the balance straight.

O.G.S.C.

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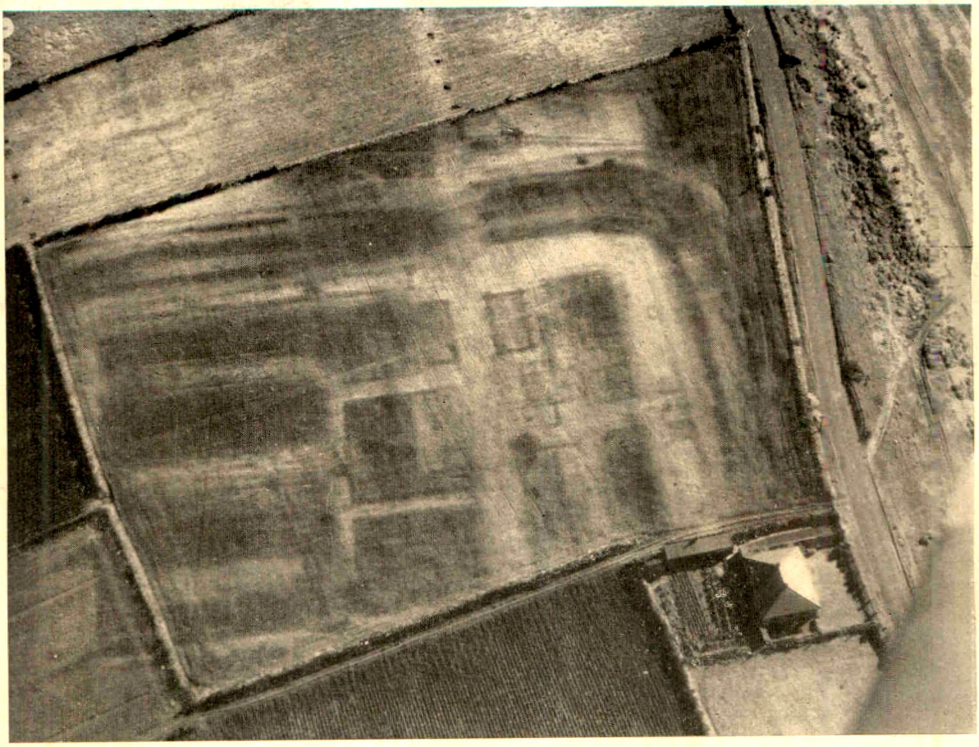
* But *not* by the Ministry of Works—for then no account would be published.

THE CALENDARS OF ANCIENT EGYPT. *By* RICHARD A. PARKER. (Studies in Ancient Oriental Civilization, no. 26). *University of Chicago Press*, 1950, 83 *pp.*, VI *pls.* $6.00.

There have been many studies dealing with the Egyptian evidence for ancient chronology; in none of them have the mysteries surrounding some known cases of a double dating, one by the civil year, and one by lunar months, been clarified. Professor Parker in this study proves that the lunar month began always with the first day on which the old crescent was no longer seen. This demonstration fortunately removes the complicated set of hypotheses resulting from calculations which used sometimes the first day of invisibility, sometimes the day of the new crescent, and confirms the coherence of the Egyptian way of reckoning time. The day started with dawn, and the observation governing the month must have been made at dawn. In proving this point, a neat and convincing explanation of the real significance of the demotic papyrus Carlsberg no. 9, published by O. Neugebauer and A. Volten, which gives a 25-year cycle of civil years in which the lunar years contain 9 intercalations, brings evidence of great importance for serious students of chronology.

The luni-stellar year originally used in Egypt can be identified with the lunar calendar of the Ebers Papyrus, and Parker ingeniously uses the Ramesseum and Senmut ceilings to develop his view that the intercalary month was named after Thoth. In an important excursus he deals with the chronology of the 12th Dynasty. Though the dates arrived at by Lynn Wood were correct, they are only so because the two errors involved, reckoning by the new crescent and attribution of certain lunar dates to the wrong reign, cancel one another.

This brochure is a masterly piece of clear exposition. Though it is hardly to be hoped that it will stop some of the flood of ill-informed writing on this subject, Parker's work leaves no excuse for some of the ill-digested schemes of chronology recently published.



ROMAN FORT, BECKFOOT, CUMBERLAND

Journal of Roman Studies, 1951, plate IV, 2

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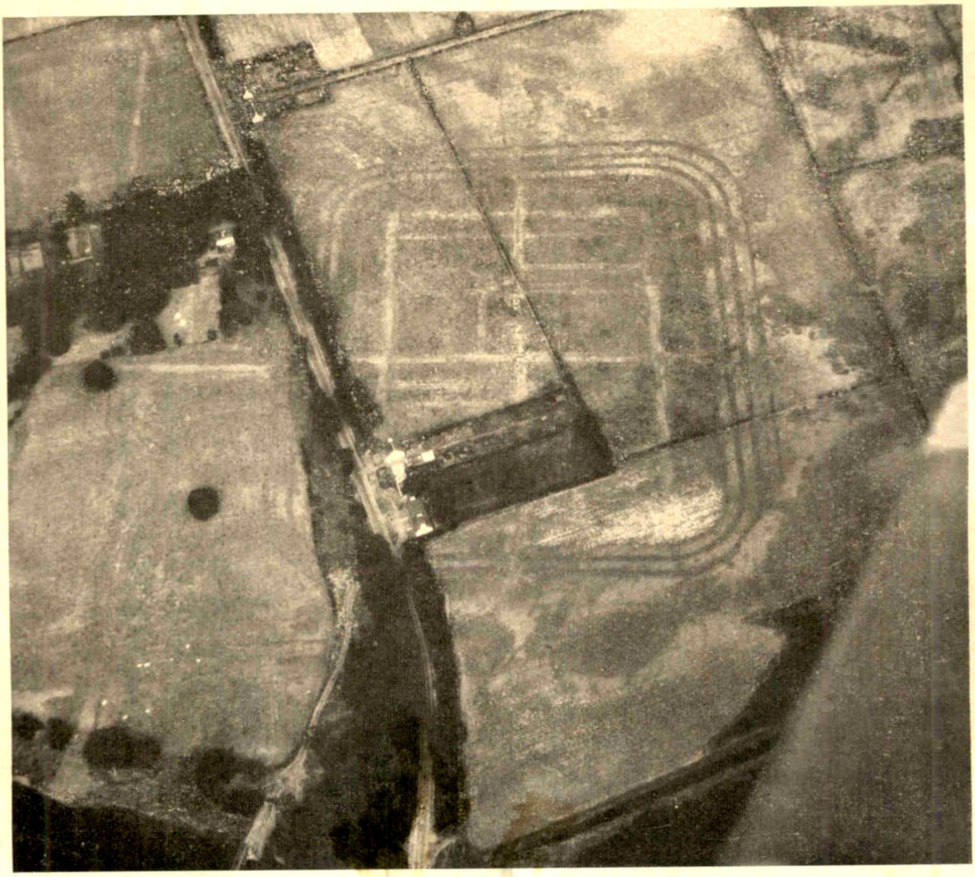

ROMAN FORT, GLENLOCHAR, KIRKCUDBRIGHTSHIRE

Journal of Roman Studies, 1951, plate VII, 1

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Antiquity

VOL. XXVI No. 102 JUNE 1952

Editorial Notes

READERS of ANTIQUITY sometimes complain that we do not publish more articles about British prehistory which provided the material for some outstanding contributions during our first decade. The explanation will emerge, we think, from the answer to a question : What is it that, in the last resort, determines the contents of a number of ANTIQUITY ? It is not merely the Editor's personal interests, though naturally these must influence his decisions. The ultimate determining factor is rather the state of archaeology in any particular region. Archaeology is an art which employs a scientific technique, and like all arts it has periods of ebullience when (to quote the dictionary) new discoveries ' issue forth in agitation, like boiling water ', and others when hardly a ripple disturbs the surface. We are certainly not in the latter stage, but has the water gone off the boil ? Most certainly it has not. New discoveries of first-rate importance, such as the mesolithic habitation-site at Star Carr, continue to be made and are exploited by the most advanced techniques. The essential difference between now and two or three decades ago is surely that the blank periods in British prehistory have largely been filled in. The work of three or four prehistorians has enabled us to see in outline the changing scenes of life for some six or seven thousand years. There are still a few blank periods such as the later mesolithic and the Middle Bronze Age, but the others are now well established.

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What remains therefore is the hardly less exciting task of employing new techniques, such as air-photography and radio-carbon dating, to amplify the picture and correct its outlines. The former, now 30 years old, is perhaps hardly quite new, but the conditions of its employment have delayed rapid progress. Thanks to an excellent cooperation between Cambridge University and the Royal Air Force a big blank on the map of Roman Britain is now being filled in. Nine years ago I forecast ' startling discoveries ' in sw Scotland, quoting Tacitus' remark that in A.D. 82 Agricola ' manned with troops that part of Britain which faces Ireland ' (*Topography of Roman Scotland*, 130). On the Ordnance Survey Map of Roman Britain that region has no Roman sites at all. Now Dr St. Joseph has begun to fill it in, and has published an excellent general résumé of his discoveries. Taking full advantage of the drought of 1949 he has found and photographed from the air a number of Roman forts and marching-camps in the valleys of the Nith and the Dee and carried the evidence of Roman military penetration as far west as Gatehouse of Fleet. It is now certain that the counties of Ayr and Wigtown must also contain Roman forts, for the advance would not have stopped short of the sea coast. By kind permission of the Editor and himself we reproduce here (*facing* p. 57) two of the air-photographs illustrating his article (*Journal of Roman Studies*, Vol. XLI, 1951). One shows the plan of an entirely new fort at Glenlochar, Kirkcudbrightshire, with its triple ditches and streets; the other reveals in startling detail the interior buildings of an already known Roman fort at Beckfoot in Cumberland. Both are revealed by crop-marks, in the latter oats. In the middle of the latter photograph, beside the road, can be seen the stone barn with its buttresses. In all Dr St. Joseph has discovered 'six new large forts, nineteen small forts, eleven signal stations or turrets, about sixty temporary camps, detailed plans of several extramural settlements, as well as of five forts, while at almost every major site . . . air photography has contributed fresh information'. This is a record of which British archaeology may well be proud; here certainly is no lack of ebullience.

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In excavation, too, Scotland has been very active, as the 6th Report of the Scottish Regional Group shows (copies of this and the next Report to be obtained from Mr R. W. Feachem, F.S.A., 3 South Bridge, Edinburgh 1, for 6d.). Much of it is done with student labour and served for training purposes. We have indeed travelled far from the times, nearly half a century ago, when Hadrian Allcroft lamented the neglect of our native sites. We have not come near to exhausting them, however, for as technique advances we are able to extract more and more information from each of them; indeed the time is ripe for the re-excavation of some of the more important ones, employing our newly acquired skill to amplify the older results. It would be well worth while thus to re-excavate some of Colt Hoare's barrows on Salisbury Plain. But as our knowledge of prehistoric and Romano-British archaeology increases and the blanks are filled, it is likely that excavations in Britain will tend more and more to become training-grounds for archaeologists whose major activities are destined for work elsewhere. The 'painstaking analysis of our ultimate prehistoric slums' is a good preparation for those who will proceed to uncover 'major civilizations and cultures of world-wide significance'. Egypt and India have already profited by such a training; the principles of excavation and field archaeology are the same in all continents.

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But ANTIQUITY has also to consider the interests of a large circle of readers, some of whom are not particularly interested in British archaeology. Enthusiasm is infectious, and it may well be that the current enthusiasms of whole-time archaeologists make the most interesting reading. What is it that arouses such enthusiasm? Chiefly it is the opportunity of using the skill they have acquired by training in a region where it has not previously been employed. That always results in new discoveries, and it is the making of discoveries that is the salt of archaeology. To judge from the questions one is asked most people seem to imagine that the archaeologist spends his time going round looking at well-known sites! Actually it is the ones which are not known that attract him. Nothing is so boring as rubbernecking round ruins; nothing so exciting as to set out on a voyage of discovery in a region that has not previously been explored, knowing that all ahead is new and unknown.

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These things being so, it is inevitable that some of the best copy should come from marginal areas in Africa and Asia. Africa is particularly ebullient at the moment from Khartoum to Cape Town, via Kenya and Angola. With a clear field and exciting new problems to solve some dozen or so enthusiasts are reconstructing the history of the human race in the continent where it may have originated. Others (including the writer) are concerned with fascinating by-ways such as the Southern Christian Kingdom on the Middle Nile, whose history is almost a blank. It is good news that the Commissioner for Archaeology in the Sudan, Mr Peter Shinnie, F.S.A., is starting a new periodical publication (to be called KUSH we believe) in which the many new discoveries made recently will be recorded. He himself has just completed the second and last season's work at Soba, the capital of the Southern Christian Kingdom of Alwa which came to an end (perhaps by gradual decay and abandonment) somewhere between 1317 and 1522.

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In the older countries of Europe—older, that is, in archaeological experience—it is an age of maturity and achievement rather than of pioneering. Enough has now been done for big books to be written, summing up the evidence. Such is the fine work of Dr J. G. D. Clark which has just appeared (see p. 86 for full title). In keeping with the times it is concerned with the basic facts of prehistoric life. That in itself is evidence of the maturity of the discipline; the scaffolding is now discarded and we can see the building.

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The Scottish Regional Group of the Council for British Archaeology is planning to bring into being an annual Summer School in Archaeology. The first session will be held in Dundee this year, from 8th to 12th August, and the subject chosen as a theme for the Session is 'The Problem of the Picts'. It is intended to hold the School in a different part of Scotland each year, the subject in each case being appropriate to the area selected. For example, the 1953 Session is planned for Dumfries, the theme being 'Roman and Native in Southwest Scotland', while in succeeding years other centres, such as Inverness, Perth or Glasgow will be chosen.

Particulars of the 1952 School may be had from the Hon. Secretary, Scottish Summer School in Archaeology, 3 South Bridge, Edinburgh, 1.

Stratigraphic Excavations in the Forum Romanum

by EINAR GJERSTAD

(*Lund University*)

IN 1903 and 1904 Giacomo Boni undertook a stratigraphic excavation to the N.W. of the foundation of *Equus Domitiani* in the middle of the Forum Romanum. The finds, unfortunately never published, are preserved in perfect order in the storeroom of the Forum Museum; and thanks to the generosity of Italian colleagues of whom I wish to mention in particular Professors P. Romanelli and S. Puglisi, this material was placed at my disposal for study and publication. When it turned out to be necessary to carry out a supplementary excavation, permission was readily granted me by the Italian authorities.

Immediately N.W. of *Equus Domitiani* I excavated an area, 5.90 m. in length and 3.40 m. in width, down to virgin soil reached at a depth of 5.87 m. below the Forum level of the Empire. When the excavation was finished there remained an earth wall in which the story of the centuries could be read in the superimposed strata.

Fig. 1 shows the section of these strata. Three categories of layers are easily distinguished. The first category comprises layers 1–19 representing successively raised pavement-levels of the Forum Romanum with earth filling below the pavement-beds on which the slabs, now missing, once rested. In the section the beds are indicated in dark shade while the fillings are in lighter colour. Layer 2 is the bed for the Forum pavement of the Empire and layer 1 is a thin adjustment stratum of earth necessitated by the fact that the covering slabs of travertine were of unequal thickness, as shown by the section. Layers 3–19 are fillings and beds for six pavements. The filling of the first pavement counted from below, i.e. layer 19, is very compact and contains numerous pieces of chipped tufa blocks, and it can be seen that an entire block had also been thrown into the filling. I shall return to an explanation of this phenomenon. The pottery found in the filling indicates that this pavement was laid *c.* 450 B.C. Most of the filling below the second pavement consists of burnt remains, giving evidence of the devastation caused by the Gallic invasion in 387/6 B.C. This second pavement seems to have been laid very soon after the catastrophe and may therefore be assigned to *c.* 385–380 B.C. Below the third pavement there was pottery of the 4th century B.C. The stratification on the Comitium has shown that the pavement there which corresponds to the third Forum pavement, may be placed in chronological connexion with the remodelling of the Comitium and the Forum Romanum undertaken by C. Maenius, and, in consequence, we may assign the third Forum pavement to *c.* 335 B.C. On the evidence of the potsherds found below the fourth pavement its date can be determined at *c.* 200 B.C. The fifth pavement was laid in the time of Sulla and the sixth pavement belongs to the years about 50 B.C. when extensive innovatory works were carried out in the Comitium and the Forum Romanum; in the former place the works were supervised by Faustus Sulla after the destruction of the Curia Hostilia by fire in 52 B.C., while the changes in the Forum Romanum are usually connected with Caesar. Thus it can be stated that the six pavements of the Forum considered above belong to the Republican period. On top of

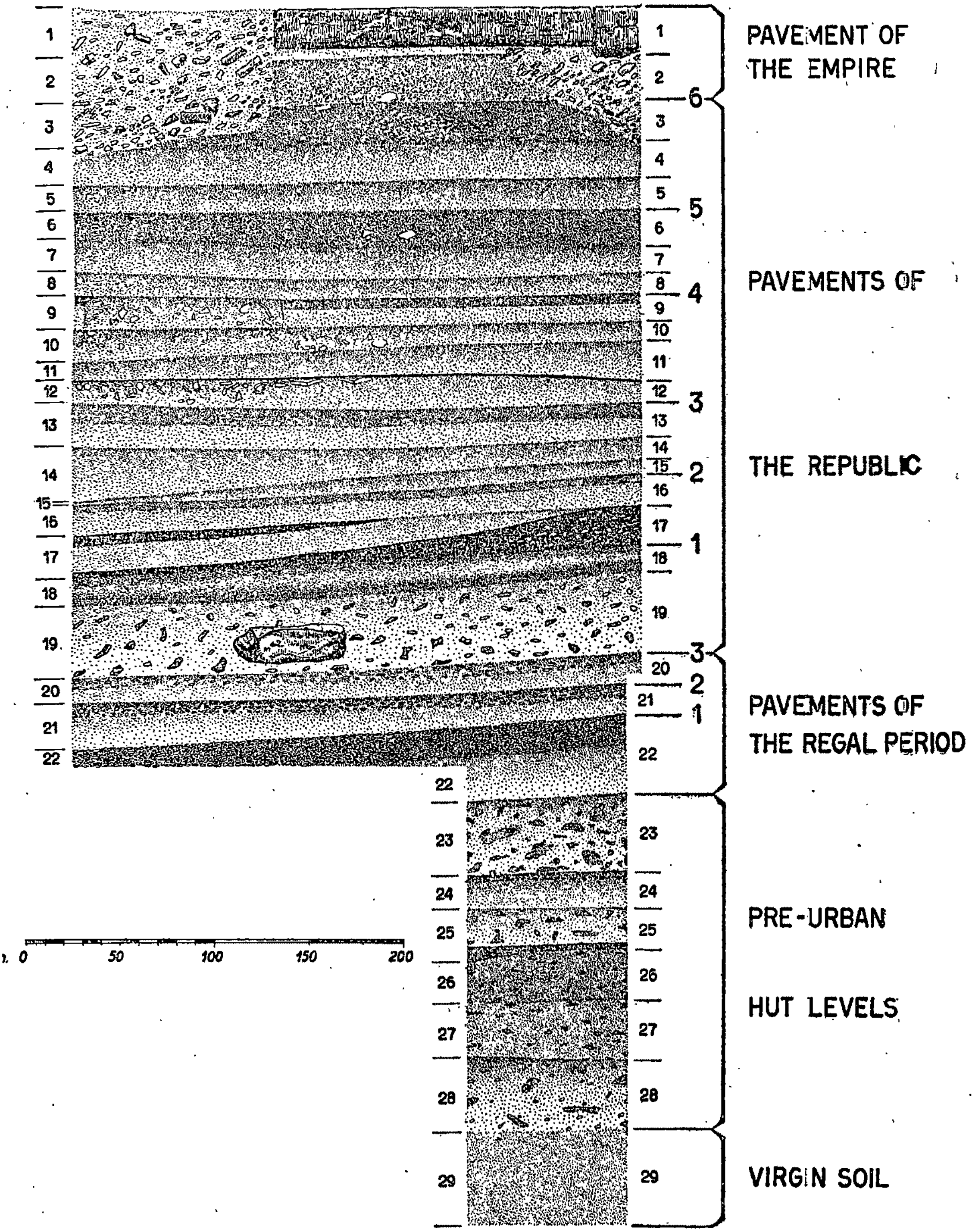

Fig. 1. SECTION OF THE STRATIGRAPHIC EXCAVATION IN THE FORUM ROMANUM

the sixth Republican pavement is the pavement of the Empire constructed during the reign of Augustus.

Continuing the study of the section we proceed to the second category of pavements of the Forum Romanun, comprising layers 20–22. These pavements are of a construction quite different from that of those described above, consisting of a bed of pebbles resting on a filling of earth. They date from the period of the Kings. The first pavement, layer 22, was laid in *c.* 575 B.C. as shown by the pottery found in the earth filling below it. The third pavement, layer 20, can be assigned to the beginning of the 5th century B.C. and lasted until the middle of the century when, as mentioned above, the first Republican pavement was laid. Remains of a wall built of well-dressed tufa blocks belong to this pavement. Evidently the wall formed part of a monumental structure of some kind, but no literary tradition about that structure seems to have survived, and it cannot therefore be identified. It was oriented to the cardinal points and was intentionally destroyed when the first Republican floor was laid, pieces of the chipped blocks having been used as filling material below that floor (*cf.* above).

The third category of strata is represented by layers 23–28, of which the lowest rests on virgin soil, layer 29, consisting of alluvial clay. Layers 23–28 are not remains of Forum pavements but of huts : the walls were built of reeds reinforced with clay (wattle and daub) and the roof-beams were supported by wooden poles. This is proved by finds of burnt lumps of clay with impressions of reed and by preserved remains of hut-floors in which there were holes for the wooden poles supporting the roof-beams. In the hut-layers were found numerous fragments of pottery, fire-supports, reels, and spindle-whorls of terracotta, carbonized grains of wheat and beans, and animal bones of cattle, sheep, and swine. Two habitation periods can be distinguished, the first one comprising the time from *c.* 650–625 B.C. and the second one extended from *c.* 625 to *c.* 575 B.C. when the huts were razed to make room for the first Forum Romanum.

This stratigraphic excavation has yielded chronologically fixed points for the history of habitation in the centre of Rome and, in addition, a primary material elucidating the pre-urban period of Rome and the foundation of the city. Other researches have already indicated what we now know : that which later became Rome was originally a group of primitive hut villages on the Palatine, Caelian, Esquiline and Quirinal hills. These hills were occupied by settlers from the 8th century B.C.[1]). The type of civilization represented by these hut villages may be characterized as local variants of the early Iron Age culture in Latium—rustic, primitive and sequestered, having little or no contact with foreign culture. Between the hills there was uninhabited ground that was partly used for burial purposes. This topographical isolation favoured a cultural peculiarity as illustrated by ceramic evidence, and a political particularism, of which there are traces left in the cult and sacred law. The stratigraphic excavation in the Forum Romanum illustrates the next stage of the development. About 650 B.C. there began to be little space on the hills for the increasing population and, in consequence, they had to settle on the slopes of the hills and still further down in the valley between these, as far as down to the area of the later Forum Romanum. The influence, then beginning, of Etruscan culture produced an increasing standardization of culture, and as a result of the topographical isolation being broken a certain collaboration in cult and politics between the village communities was organized. In literary tradition this stage of development is reflected by the festival called Septimontium, celebrated by the inhabitants on seven hills within the area of the Palatine, Caelian and Esquiline, while the Quirinal was excluded from it. The festival was not celebrated in common by the population within this area but by the inhabitants of each hill separately, each hill having its own organization for this festival, its own *magistri* and *flamines* as shown by later inscriptions. The organization of this festival does not therefore indicate a political fusion of the communities on the Palatine, Caelian and Esquiline, but rather, at most, some sort of confederation based on connexion in cult, representing a transitory stage between the isolated village settlements and the unified city, Urbs. I must confess that I have previously been sceptical about the Septimontium stage, if I may call it so, in the pre-urban history of Rome and I have reckoned with the possibility that we had to do with a case of learned speculation. The archaeological evidence produced by the Forum excavation has, however, convinced me that the Septimontium stage is not a fiction but a concrete reality.

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[1] On the Caelian Hill no systematic excavations of the earliest strata have been carried out ; only remains of the Empire and of the Republican period have been examined. The fact that the Caelian hill was one of the *montes* celebrating the pre-urban festival of Septimontium (cf. below) shows that the habitation of this hill must go back to the pre-urban period, but how far back can only be ascertained through excavation.

About 575 B.C. an epoch-making event occurred and there cannot be any doubt about its significance. The primitive hut settlements came to an end and on the top of the razed huts the first Forum Romanum was laid out. The period of the villages, that of the pre-urban villages, is gone. The earlier villages, those of the Septimontium and of the Quirinal, are united into one community, a city, and the point where they all met was occupied by a market-place, the Forum, the political and economic centre of the new city. This was the foundation of Rome. The results of the Forum excavation are confirmed by other facts. On the Capitoline extensive soundings have been made below and near the temple of Jupiter Optimus Maximus, between this temple and the Portico del Vignola, on the Piazza del Campidoglio, at the temple of Veiovis below the Palazzo Senatorio and in many places along the *Clivus Capitolinus.* Nowhere has a stratum or a deposit been found that is earlier than the second quarter of the 6th century B.C. This is a conclusive result showing that the habitation of the Capitoline is not earlier than about 575 B.C. At that time the Capitoline hill, hitherto uninhabited, was chosen as the *arx* of the united city. At the eastern boundary of the Forum, opposite the Capitoline, two buildings were erected, which—like the Capitoline—are intimately connected with the foundation of Rome both in sacred and political respects, viz. the Regia, the chancellery of the rex, the sovereign of the new city and state, and the temple of Vesta, the sacred symbol of their persistence. Both these edifices were built at the same time as the first Forum Romanum was laid out, as proved by the earliest pottery connected with them. Thus the work of political unification was accomplished : Rome had been founded. *Ab urbe condita* is the time after *c.* 575 B.C. That is the initial date of the Regal period.

The Forum excavation has also contributed to the solution of the final date of the Regal period. The traditional date for the introduction of the Republic, 509 B.C., is a fiction, as shown recently by Krister Hanell in his work 'Das altrömische eponyme Amt' (*Acta Inst. Rom. Regni Sueciae*, Ser. in 8°, II, 1946). A critical study of the literary tradition leads to the conclusion that the transition from the Regal period to the Republic should be assigned to about 450 B.C. and this conclusion is confirmed by the archaeological material. During the Regal period Rome was dominated by Etruscan civilization. The year 509 B.C. does not mark a change in the cultural situation and Etruscan art continued to flourish in Rome until about the middle of the 5th century B.C. At that time contact with Etruscan culture was suddenly broken. The result of the Forum excavation points in the same direction. The date of the pavement represented by layer 20 approaches the traditional date for the introduction of the Republic, but that pavement belongs to the same structural category as the first and second pavements of the Regal period. It does not introduce a new period in the history of habitation in Rome, but represents the concluding phase of an old one, the third phase of the Regal period. A new structural type marking a break in the history of habitation is introduced by the pavement which I have called the first Republican pavement and that can, as mentioned, be assigned to the middle of the 5th century B.C. Full documentation of the opinions here presented will be given in my work ' Early Rome ' to be published in the *Acta* of the Swedish Institute in Rome.

Finally I wish to summarize the evidence brought forward by the Forum excavation for the determination of the principal periods of the history of habitation in Republican Rome. I have already treated that subject in connexion with my investigation of the architectural history of the Comitium and I refer the reader to that paper, ' Il Comizio Romano dell' età repubblicana ', published in *Opuscula archaeologica* II (*Acta Inst. Rom. Regni Sueciae*, V, 1941, pp. 97 ff.). The six architectural epochs of the Republic determined on the stratigraphic material from the Comitium agree with the six periods distinguished on the stratigraphic evidence from the Forum Romanum. The first Republican period comprises the time between *c.* 450 and 387/6 B.C. when the Gauls invaded Rome, devastating the city. After the Gallic invasion a new period begins, marked by the second Republican pavement, a period of restoration and concentration of power against external and internal dangers: the Republican city-wall and the temple of Concord erected after the enactment of the Licinian laws are monuments characteristic of the time. The victory in the Latin war in 338 B.C. inaugurated an important architectural period, its buildings in the Forum Romanum belonging to the third Republican pavement. Latium became Roman and, through the treaty with Capua, Greek civilization was allied to Rome, and the incipient influence of early Hellenism is apparent in Roman temple architecture of the 3rd century B.C. After the second Punic war Hellenistic culture penetrated into different branches of Roman life, a necessary consequence of the world-politics on which Rome embarked. The fourth Republican pavement of the Forum was one of the scenes for this cultural process. The Forum Romanum was rebuilt in monumental style: in 184 the Basilica Porcia was erected, in 179 the Basilica Aemilia, and in 170 B.C. the Basilica Sempronia. This urbanistic architecture of Rome represents an artistic synthesis of Italic individualism and Hellenistic form. After Sulla's return from the campaigns in the East this architectural process was accomplished during the time corresponding to the fifth Republican pavement of the Forum. The short period represented by the sixth and last Republican pavement is the Caesarian prologue of the Empire: in this period, or, more exactly, in 54 B.C. Caesar's forum, the Forum Julium, began to be built, inaugurating the series of Imperial fora.

What was Murrhine?

by C. N. Bromehead

In 1855 C. Müller[1] published the first volume of his *Geographi Graeci Minores*: in the introduction to the 'Periplus of the Erythraean Sea' he states that he has studied six hundred writers on the question 'what was murrhine, but that it may be now regarded as certain that murrhine vases were made of fluorspar'. Since, however, the opinions which Müller discarded have been revived by writers entitled to respectful consideration, some of whom have also given reasons for rejecting the identification with fluorspar, it seems desirable to review the matter and to put on record certain new evidence.

The first stage in the enquiry must be to decide whether murrhine, i.e. the Greek and Latin *murrha* or *morria*, was a natural substance or an artificial product. The late B. Laufer[2], of the Field Museum of Natural History, Chicago, has adopted the latter view and argued the case at length; he maintains that some Chinese pottery with coloured glazes, discovered by him, attributable to the Han period (say 3rd century A.D.) is murrhine ware. In his discussion he necessarily lays great stress on the line in Propertius (IV, 5, 26) 'murreaque in Parthis pocula cocta focis' which he translates without comment as 'murrine goblets baked in Parthian furnaces' (p. 126); on another page he puts 'kilns' for 'furnaces', but this may be a quotation from a German rendering. Even if Propertius intended to state that the cups were artificial products from a kiln, to accept him as against the clear evidence of Pliny seems like accepting Coleridge as against, say, Herschel for the presence of a star within the horns of a crescent moon. That Pliny regarded murrhine as a stone is absolutely certain, and that he was so understood throughout classical times is confirmed by Isidorus (*Etym.* XVI, 12); in one passage (XXXIII, 2) Pliny says that from this same earth (i.e. that supplies metals) we have dug up (*effodimus*) vessels of murrhine and vases of crystal; in another (XXXV, 46) he contrasts murrhine and crystal with earthenware (*fictilia*). Laufer translates *fictilibus simpluviis* as 'plain earthenware ladles' (p. 134) and says 'the contrast intended by the author between the rustic unglazed indigenous Italic earthenware and the pretentious, glazed, imported Oriental pottery is self-evident. Pliny, further, includes emerald, gems and murrhine as the most valued products of the earth, comparable with diamonds (XXXVII, 78); Laufer writes (p. 134) 'in my opinion it [this passage] is of no consequence'. He ignores completely Seneca's classification of *murrhina pocula* as *gemmae* (*De Ben.* VII, 9).

The use of the word *focus*, however, is sufficient to show that Propertius's language is poetic: *fornax* would be more natural for a kiln, but even that is used by poets for the internal fires of Aetna. Pliny states that murrhine is supposed to be a vapour condensed underground by heat (*humorem putant sub terra calore densari*, XXXVII, 8) in contrast with rock-crystal which was ice made permanent by excess of cold. This passage is quoted by Isidorus and expresses the common opinion on the origin of most minerals, an opinion which lasted throughout classical and medieval times ; only that on the origin of crystal can be denied today. Why should not the ' Parthian hearths ' refer to this theory ?

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[1] Müller, C., *Geographi Graeci Minores*. Paris, 1855.

[2] Laufer, B., *The Beginnings of Porcelain in China*. Museum of Natural History, Chicago: Anthropological series, vol. XV, no. 2, 1917.

Even if there were any definite statement in a classical author that murrhine was artificial, it would be incredible and we should have to regard it as on a par with the view that crystal was a form of ice. The first murrhine vases to reach Rome were carried in Pompey's triumph in 61 B.C. The latest use recorded is that by Elagabalus, whose extravagances were so great that ' *in murrinis et onychinis minxit* ', a passage often quoted, though I have seen no allusion to the more telling statement that M. Aurelius sold his murrhines to pay for the war with Germany. Throughout this period murrhine vases are mentioned only as the greatest rarities, fetching fabulous prices. One of the most notable features of the early empire was the rapid growth of trade between Rome and the east. Pottery that could fetch such prices as the 300 talents given by Nero for one murrhine cup would inevitably have reached Rome in increasing quantity, though there would of course have been a corresponding fall in value. Nothing of the sort occurred. Murrhine must have been a rare mineral found only in small quantity and soon exhausted.

It seems unlikely that any modern writer would have seriously considered equating murrhine with any form of pottery, had it not been suggested by the old claim that murrhine was Chinese porcelain. This was first put forward by Cardan in the middle of the 16th century (*De Subtilitate*, IV) and was supported by Scaliger, who usually opposed him, followed also by Pancirollus, Hofmann and many others. The first reasoned opposition came from De Boodt[3] (Bk. II, chap. LXXXV and XCII) who favoured Sardonyx or Onyx. The porcelain theory is now known, as Laufer himself admits, to be historically impossible, and appears to be founded on a chance analogy. In classical times vases and cups called murrhine reached Europe from the east ; they were greatly valued for the beauty and variety of the colours, were translucent and fragile. Chinese porcelain reached Europe in the 16th century, and the same words apply. ' Therefore ' the two materials were the same! Laufer's glazed pottery preserves this long-held view with the minimum of correction and the identification with murrhine enhances the importance of his discovery.

We may now approach the second part of the problem. Assuming that murrhine was a natural mineral, what mineral was it ? Neither jade, soapstone, amber nor obsidian, all of which have been suggested, seem to call for serious consideration. The two important identifications are with agate and with fluorspar. ' Agate ' is a convenient and familiar name for various banded forms of chalcedony, a colloid form of silica, which in the crystalline state is quartz or rock-crystal. Various names have been given both in classical and in modern times according to the colours of the mineral and the arrangement of the banding—onyx, sard, sardonyx, cornelian and others.

In 1546 Agricola[4] definitely identified the murrhine of Pliny with onyx (Bk. VI, pp. 296, 7) using, however, the word ' chalcedonius ' in his glossary of German equivalents for Latin names (p. 481). De Boodt, in 1609, under the heading Sardonyx, says ' I consider that the murrhine vases so greatly esteemed in ancient times were made of this gem and were not what is now termed porcelain, brought from China ' ; and under Onyx : ' among the Roman vases of onyx were called murrhine '. One argument on the other side is given by Hofmann,[5] *s.v.* Myrrha—if the murrhine vases of the ancients were made of onyx, why were they called murrhine, or what reason can be given for this appellation, and why are murrhines everywhere distinguished from onyx ? He favours porcelain, but adds the cautious statement that neither the ancient authors nor their commentators knew what murrhine was. The various forms of chalcedony and agate had, of course, been familiar to the Greeks and Romans for centuries. Achates and onyx are given by Theophrastus and, with sard, in the Septuagint; many other varieties are mentioned by later writers. Pliny describes many colour-varieties, moss-agate, etc., mostly under compound names such as dendrachates for the last. It is certain that, had murrhine been any form of agate or chalcedony, he would have recognised and described it as such.

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[3] de Boodt (Boethius), A., *Gemmarum et Lapidum Historia* (1st ed., 1609) ; 3rd ed. Leyden, 1647.

[4] Agricola, G. (=Georg Bauer). *De Natura Fossilium Libri Decem.* Basle, 1546.

The greatest authority to favour agate was Rev. C. W. King,[6] and it was probably his influence that led Professor K. C. Bailey to accept this identification and the new Liddell and Scott to give ‘ perhaps agate ’ under the Greek *μόρρια* (used by Pausanias) and the forms of *μορρίνη* in the Periplus of the Red Sea. It is strange that King himself should state very clearly, in favour of this theory, one of the strongest arguments against it (p. 182) :—‘ Fragments of bowls made of Agate (but of no other irregularly coloured stone) are turned up in abundance in the soil of the ancient capital, and often of a radius that bespeaks the extraordinary circumference of the perfect vessel. Such pieces, if not large enough to be preserved as antiques, are cut up into brooch-stones and every spring furnishes the Roman lapidaries with an inexhaustible supply. Perfect vessels . . . are rare, yet a comparatively large number are yet in existence ’. An ‘ inexhaustible supply ’ of fragments of murrhine is a flat contradiction of every classical allusion to the substance without exception. The identification with agate is tentatively accepted by Bailey[7] (Pt. I, p. 176), who writes ‘ it is doubtful whether the identity of this substance will ever be certainly known. The three main theories are (a) that it was porcelain, (b) that it was fluorspar, (c) that it was some variety of silica, such as onyx, opal, or agate, probably the latter. The first theory . . . may be unhesitatingly rejected . . . on the whole the opinion of C. W. King and Marquardt that agate is the substance in question is probably correct ’. Bailey's objections to fluorspar are dealt with below.

It is, moreover, impossible that agate vessels should have ever approached the play of colours ascribed by Pliny to murrhine—purple, white, a fiery red, etc. The modern practice of colouring agate artificially may, perhaps, mislead classical scholars : it is true that, in the Bombay district, the natural colours of agates have been deepened by roasting at least since the 1st century A.D. ; but these are only white, yellow, brown and reddish ; blue agates are produced by a modern chemical process.

A magnificent set of eight agate vases, from the description and illustrations probably of roasted Indian agate, were found near Qift in the eastern Egyptian desert and published by Englebach.[8] They had probably entered Egypt at Berenike or Myos Hormos. These are mentioned by A. Lucas in the last edition of his *Ancient Egyptian materials* (p. 443), who adds ‘ these probably came from India and are "murrhine vessels", such as Pliny describes. At a late date agate and onyx beads were imitated in glass'. We learn from the Periplus (Para. 6) that glass imitations of murrhine (μορρίνη λιθια as opposed to μόρρα) were made at Diospolis, probably Thebes, and sent to the Red Sea ports, apparently for export to the east. The manufacture of glass to imitate vessels of murrhine, hyacinthus and sapphirus (sapphire and lapis lazuli) is mentioned by Pliny (xxxvi, 198). First century glass in imitation of agate is by no means uncommon and specimens that any mineralogist would regard as imitations of fluorspar by no means unknown.

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[5] Hofmann, J. J., *Lexicon Universale . . . metallorum, lapidum, gemmarum nomina, naturas, vires.* Leyden, 1698.

[6] King, C. W., *Natural History of Gems or Decorative Stones.* London, 1867.

[7] Bailey, K. C., *The Elder Pliny's Chapters on Chemical Subjects.* London, vol. 1, 1929 ; vol. 2, 1932.

[8] Englebach, R., Recent acquisitions in the Cairo Museum. *Ann. des Serv. des Antiquités de l'Egypte.* Tom. XXXI, 1931, pp. 126–31 and plate.

The identification of murrhine with fluorspar was first suggested by F. Corsi[9] in 1825 in the catalogue of his collection of ancient marbles and stones. In his book published in 1845 he gives as his main reason, that this mineral alone agrees with Pliny's description, and then argues the case at length, referring to every classical allusion and to the chief commentators (pp. 166–95). The same conclusion was stated, apparently independently, by F. Thiersch. It has already been mentioned that C. Müller accepted this view and regarded the murrhine question as closed.

It therefore remains to answer the objections put forward since that date and to add certain new scraps of relevant information. As far as I am aware none of the objectors has any claim to be a mineralogist, surely a necessary qualification for the subject. One objection is raised by Bailey (ii, p. 176) and, less clearly, by Laufer:—'The greatest objection to this identification is the fact that no mention is made of the crystalline form of fluorspar. The least observant, on inspecting a collection of fluorspar, must be struck by the very perfect cubes in which the substance almost always crystallizes. The form is so simple and striking that it is hard to believe that Pliny, while describing accurately the form of rock-crystal (xxxvii, 26) would have omitted altogether to describe that of fluorspar'. It is true that specimens of fluorspar exhibited in a museum usually show the cubical crystals very clearly—they are selected for that purpose. But Pliny never saw the natural mineral, only vessels carved from it. For this purpose the massive variety, with colour-banding, would necessarily be used. A collection of wrought fluorspar, whether Derbyshire Blue-John vases, German ornaments, or Chinese vessels and figures, would scarcely every reveal the cubic structure.

Another point arises from Pliny's statement (xxxvii, 7) that 'a person of consular rank, who some years ago used to drink out of this cup, got so passionately fond of it as to gnaw its edges, an injury, however, which has only tended to enhance its value'. Laufer seems to think that it would be impossible to gnaw a cup cut from 'agate, fluorspar, or any other stone with which these vessels have thoughtlessly been identified. Agate, which has a hardness of about 7 degrees and, having no cleavage, is tough, would certainly need to be very thin: yet many children have bitten pieces out of glass tumblers. Fluorspar, on the contrary, has a hardness of only 4 degrees, slightly less than the enamel of a human tooth, a cleavage which is mineralogically described as perfect, and is brittle: it is very easy to nibble little pieces off the rim of a fluorspar vase (*experto crede!*). In fact Pliny's use of the word *fragilitas* (xxxiii, 2) the gnawing of the cup just mentioned, and the many allusions to the brittleness of murrhine by other authors, and again the reflections of rainbow colours near the edges (xxxvii, 8), due to incipient cracks along cleavage planes, all favour fluorspar.

The massive mineral is translucent, never transparent, and may in parts be almost opaque. This property is referred to by Martial (iv, 85)—it would be quite impossible to distinguish two different wines when seen through a fluorspar cup—and may be the meaning of Pliny's phrase ' *Splendor his sine viribus, nitorque verius quam splendor* (xxxvii, 8).

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[9] Corsi, F., *Delle Pietre Antiche Trattato.* Rome, 1845.

Mention is also made of a pleasant odour from murrhine cups used for drinking wine. Laufer considers that this is intelligible only if the question is of pottery, ' scented minerals or glass are not conceivable ' : yet in a footnote on the same page (131) he refers to pottery which derives its scent from the mineral, a ' reddish odoriferous clay ' from which it is manufactured. Carved fluorspar may well have a slight odour or impart a faint taste to wine, though this is not due to a natural property. In the process of cutting and polishing Derbyshire fluorspar, it was the custom to soak the mineral repeatedly in hot rosin in order to overcome, as far as possible, the tendency to fly to pieces. Traces of this artificially added odoriferous material can sometimes be detected now in vases manufactured a hundred years ago.

A passage to which I have not seen any reference made by any commentator on this subject is in the *Silvae* of Statius (iii, 57) :—' cups of crystal and of weighty murrhine ' (*murrasque graves*). Rock crystal has a specific gravity of 2.65, fluorspar of 3 to 3.25, so that the latter would be noticeably heavier than a crystal cup of the same bulk. Laufer does not give the specific gravity of the Han pottery which he claims as murrhine, but he describes the body of the ware as a ' porous porcelain froth ' (p. 88), presumably much lighter than crystal.

Pliny states that murrhine vessels come to us from the east ; they are found at many localities, not (otherwise) noteworthy, in the kingdom of Parthia, more precisely in Carmania. Many occurrences of fluorspar have been reported in and around this region by geologists, especially those of the Anglo-Iranian Oil Company ; one personal correspondent unconsciously uses almost exactly Pliny's words to describe the colours.

Laufer (note, p. 129) says ' it is astonishing with what high degree of tenacity the unfounded opinion of fluorspar vessels could hold its position in the face of the bare fact that no such vessels ever existed in ancient Persia, Egypt, or in classical antiquity and have never (*sic*) come to light . . .' In the *Century Dictionary* it is justly remarked under *Murra*, ' the principal objection to this theory is that no fragments of fluorspar vases have been found in Rome or its vicinity '. This statement is definitely false. Corsi mentions two antique fluorspar vases found in Rome, one in the Museo Kircheriano, which he says ' so completely answered the description of the *murrhine* that it seems as if it had been in the hands of Pliny when he wrote his description of that material : another in the possession of Signor Gillet-Laumont '. I have quoted this passage from Westropp (pp. 181, 2), not direct from Corsi, to show that the facts are available in English as well as in Italian. Middleton[10] describes the Kircheriano specimen as ' a small shallow cup made of fluorspar, which appears to be ancient ' (p. 17) : he also says that ' a few other pieces [of fluorspar] have been found in Rome '. Westropp[11] also gives a full account of eight blocks of fluorspar ' lately discovered at Rome, at the Marmorata, by Signor Visconti '. These probably came from Regensburg on the Danube and are irrelevant to the murrhine question.

It is not only at Rome, however, that vases of fluorspar have been reported. It would be reasonable to expect that some of the original murrhine vessels were in Pompeii and Herculaneum at their destruction in A.D. 79. It is stated by Egglestone that ' in excavating at the ruins of Pompeii two large " Blue John " vases of excellent workmanship were found ' ; but this tale has been officially denied : if these vases ever existed, they disappeared, perhaps by doubtful means, into a private collection soon after the reported discovery, which cannot now be verified.

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[10] Middleton, J. H., *Ancient Rome in* 1888. Edinburgh, 1888.

[11] Westropp, H. M., *Manual of Precious Stones and Antique Gems*. London, 1874.

In 1940 the late T. Sheppard published statements that on a visit to Naples he had seen two vases recently found at Herculaneum, which he identified as made of Derbyshire Blue John. While quite familiar with the Derbyshire material, Sheppard had little or no knowledge of continental or oriental fluorspar. If they have survived the war, these vases should be available for expert examination.

This paper was first put together to show that murrhine *must have been* fluorspar. Since then two vases of carved fluorspar have been described and illustrated by Loewental and Harden.[12] They are of the 1st century A.D., found near the Turco-Syrian border, one at any rate in circumstances that prove that it was a highly valued treasure. As indicated by the title of their paper it is beyond question that they were ' Vasa Murrina '. In short, murrhine *was* fluorspar.

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[12] Loewental, A. I. and D. B. Harden. ' Vasa Murrina '. *Journ. Roman Studies*, vol. XXXIX, 1949, pp. 31–7. Plates V–VII.

Inyanga: a preliminary report

by ROGER SUMMERS

INYANGA is a district in Southern Rhodesia whose monuments were brought to the notice of readers of ANTIQUITY a short while ago[1]. A Fund raised by private subscription mainly from Rhodesian sources has recently sponsored excavations in this district and at the conclusion of the second season's work it seems desirable to make a preliminary report. The excavations were directed by the writer who was fortunate in having the very skilful assistance of the Colony's Inspector of Monuments, Mr K. R. Robinson. Unfortunately no amateurs were able to assist us.

The only serious work done previously in this district was that undertaken by the late Dr Randall MacIver in 1905[2]. At that time Rhodesian archaeology was an untouched field and MacIver had no comparative material to work on, so that it was high time that something should be done to put Inyanga into its place beside other cultures of the Rhodesian Iron Age known to us through the efforts of subsequent workers, notably Miss Caton-Thompson[3]. As the area covered by the Inyanga Ruins is between 2,000 and 3,000 square miles it was rather a problem to know where to start in order to get the greatest value out of our somewhat slender resources. It may be thought that in so vast an area we should have made use of aircraft or at least air-photographs in our preliminary reconnaissance. For several reasons, limited resources not the least, we decided not to use aircraft at first but hoped to have air-photographs for study later. Through the good offices of the Surveyor-General of Southern Rhodesia we are likely to have the use of air photos of part of the area very shortly. When they have been examined further work may be suggested.

Preliminary ground reconnaissances were made in 1949 and early in 1950 from which it was clear that we had to deal with at least two cultures and probably more. Two of these seemed to be present at the *van Niekerk Ruins* where MacIver had worked, and there seemed a possibility of a third culture on the uplands around the village of Inyanga proper. During our reconnaissance we heard of, but did not visit, extensive ruins many miles north of Inyanga. We planned therefore to spend our first season at van Niekerk Ruins where we hoped to establish a cultural succession which would help us to unravel the problems of the lesser known ruins further north. We planned, too, to spend some time in the Inyanga upland Zone excavating in typical ruin groups there.

The areas in which we proposed to work varied in altitude between 3,000 and nearly 7,000 feet, and as the danger of malaria makes it impossible to camp during the wet season (November to April) it was somewhat difficult to arrange a programme which would enable us to escape the extreme heat of October in the low country and the bitterly cold winter of the uplands. Fortunately a knowledge of the country enabled us to overcome these difficulties and avoid any loss of time or efficiency from bad weather or sickness. From the beginning of June to the end of August 1950 we were continuously employed at van Niekerk Ruins and excavated on twenty different sites besides making surface collections. For part of July and August 1951 we worked over a very large area in the northern part of the district, excavating at thirteen sites and making very extensive surface collections ; finally, most of September was spent in the Inyanga Uplands where three sites only were excavated, although extensive reconnaissances were made.

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[1] Crawford, O. G. S., 'Rhodesian Cultivation Terraces', ANTIQUITY, XXIV, 96–9.

[2] MacIver, D. Randall, *Medieval Rhodesia*. London, 1906.

[3] Caton-Thompson, G., *The Zimbabwe Culture*. Oxford, 1931.

Van Niekerk Ruins proved rather a disappointment. Occupational material was extremely sparse, and six long weeks passed before a midden associated with the Ruins was located ; this is totally different from the case of ruins of the Zimbabwe Culture as students of Miss Caton-Thompson's work will know[4]. However, details of the construction were studied in a large number of cases, and for the first time in archaeological work in Rhodesia (and, I think, in all Southern Africa) a study was made of carbonised grain found in sealed deposits.

PRE-RUIN CULTURES. Despite our lack of finds in the ruins proper we were rewarded by finding traces of two pre-Ruin Iron Age Cultures : one of these comes from MacIver's ' Place of Offerings ' and the other from somewhat similar deposits a mile or two away. The pottery associated with both these cultures belongs to the very widespread class which I have described elsewhere as ' Stamped Ware '[5] and the Cultures may therefore be assigned to Rhodesian Iron Age A. Although the ceramic differences are slight they are unmistakable when large assemblages of decorated sherds are compared, but the main difference is in the associated finds. The ' Place of Offerings ' ware (which I have called *Ziwa* 1, from the name of the mountain at whose foot MacIver dug) is associated with copper and iron objects, shell beads, cowrie shell and worked bone, whilst the other ware (*Ziwa* 2) although also associated with copper and iron has none of the other objects but does have as its associates glass beads—pale blue transparent canes with sharply snapped ends.

Although the stratigraphical evidence for the relative age of Ziwa 1 was not impeccable at Niekerk it seemed probable that it was older than the Ruins. For the age of Ziwa 2 we obtained good evidence in that finds belonging to this Culture lay below huts on whose floors were sherds and other finds common in Ruin assemblages. This stratified site is on the slope of Ziwa mountain, a granite peak towering a thousand feet or more above the Ruins, and seemed to be associated with very rough granite-faced terraces quite unlike any which were associated with the Ruins ; we could not however prove the association on this site.

During 1951 however we were fortunate in finding these same Iron Age A cultures further north, and were able to prove to our own satisfaction that Ziwa 1 was indeed older than the Ruin terraces (probably much older, for a thick deposit of sterile hill wash, eroded and redeposited, lay between the terrace level and the underlying Ziwa 1 midden). Moreover, a Ziwa 2 deposit lying on, and to some extent in, rough granite faced terraces on the slope of a granite mountain was located several miles from the original Ziwa site.

There is no stratigraphical evidence for the relative age of Ziwa 1 and Ziwa 2. I have suggested the order given purely on the absence or presence of imported glass beads—a doubtful criterion and one which is liable to be upset by future work ; however the two cultures are sufficiently unlike to need differentiation, and accordingly I have made a guess at their order.

THE RUINS. Mr Crawford has already pointed out the probability that these ruins were in the main cultivation terraces[6] and MacIver, although wedded to the idea of fortification in his first book, later accepted the generally held theory of cultivation.[7] We were able to demonstrate the validity of this theory in three ways.

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[4] Caton-Thompson, G., *The Zimbabwe Culture*, Oxford, 1931.

[5] Summers, R., ' Iron Age Cultures in Southern Rhodesia ', *S. Afr. Jnl. Sci.*, XLVI, 95–107.

[6] Crawford, O. G. S., ' Rhodesian Cultivation Terraces ', ANTIQUITY, XXIV, 96–9.

In the first place all the terraces connected with the ruin complex at van Niekerk are on dolerite, a rock whose decomposition gives rise to a fertile soil very suitable for grains of the millet family. Secondly many of the enclosures at van Niekerk Ruins are partly or wholly taken up with structures too small for huts and bearing a close similarity to modern grain stores. Finally, we found in deposits connected with presumed grain stores or with querns, carbonised seeds which have been identified by Dr H. Wild of the Southern Rhodesian Division of Botany, as millets, sorghums and pulses. Dr Wild spent some time at our camp and greatly widened the scope of our work by directing our attention to various botanical points in connection with the terraces. He is of the opinion that the grain would not have grown in the often waterlogged meadows or *vleis* between the terraced hills but that on the well-drained terraces a good crop could have been reaped for several years.

The walling both of the terraces and of the buildings connected with them looks at first sight rough and unfinished, but further acquaintance with it reveals its finer points and leaves one in no doubt about the skill of builders who with the greatest economy of labour regularly used boulders weighing anything up to a ton for their building. There is no doubt that, before the van Niekerk Ruins were terraced, the dolerite hillsides were strewn with boulders of all sizes; indeed we once had the unpleasant task of climbing one such slope during a reconnaissance, and it is certain that, in order to grow anything at all, a general tidying up would have had to be undertaken. The people of the Ruins seem to have been almost unnaturally tidy, for there are little heaps of stones piled up not only in cultivated lands—a common sight anywhere in the world—but also round the base of baobab trees, round immovable boulders and at the foot of terraced hills. Such tidiness is no longer a local African trait and one is bound to suggest that it was only extreme need which led the Ruin dwellers to go to such lengths.

A further indication of the difficult conditions under which they lived is the fact that certain kopjes which contained remains of grain stores were quite clearly strongholds, completely walled and with entrances overlooked by small rooms very suitable for a guard. It seems therefore that the more important stores of grain had to be kept under military guard.

Many of the enclosures contained pits about 15 feet in diameter entered by an inclined passage in the sides of which were two holes, one as much as 10 feet deep into which a wooden bolt could be withdrawn and one shallow to form a staple into which the bolt would fit. Occasionally there is provision for locking the bolt into position by inserting a peg at its inner end. This simple bolt device was found in many places in these Ruins but is unknown elsewhere in Southern Rhodesia. In nearly every case the wooden bolt has disappeared but in one building we found one still in its original position.

Very little dating evidence was found in the Ruins but from a few beads from some four different sites it seemed probable that an 18th century date is not unreasonable. Ring counts from a number of trees of common species now growing on the terraces gave an average age of about 200 years thus confirming the bead evidence. Despite the age of the trees they are very small timber and the present inhabitants (Waunyama, a Shona tribe) have great difficulty in getting enough for fuel and building. No doubt the stone buildings were in part the response of former people to a relatively treeless environment.

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[7] MacIver, D. Randall, 'Rhodesia (note on Archaeology)', ANTIQUITY, I, 103.

In 1951 we examined a very similar group of ruins further north. These showed a number of minor differences in planning, building technique and pottery but in general the cultural picture seemed very similar to that at Niekerk. Dating evidence was just as scanty but again we recovered, at the expense of a great deal of sieving, just enough beads to convince us that both sets of ruins were contemporary. These northern ruins were probably the first to attract European attention ; a newspaper article on them appeared in 1898, and Carl Peters, a German traveller, described them in 1901, garnishing his description with the most fantastic speculations[8].

The more northerly ruins were much less encumbered with bush than those at Niekerk and we were able to study their construction more fully. Thus we confirmed our previous impression that hillsides had been terraced bit by bit apparently in accordance with the dictates of shifting agriculture, so that what one sees to-day are the imperishable remains of a century or more of continuous work, only a very small proportion of the whole being cultivated at a time. The sparseness of occupational debris is itself evidence of constant removal to new fields whilst the very ubiquity of terracing in this district shows how thoroughly the past inhabitants sought out every scrap of available arable land.

The ruins are all in lowland country between 3,000 and 4,000 feet.

THE INYANGA UPLANDS. At the end of the 1951 season we spent some time on the Inyanga Uplands—5,500 feet or higher. Here we were in an area of cold winds and almost perennial rain, totally different from the warm dry lowlands which, though only a few miles away, are in the rain-shadow of the mountains of the Inyangani group (Inyangani, 8,517 feet). Not unnaturally the cultural pattern is different. Terraces are fewer and usually on more level ground ; enclosures become circular platforms round a pit some 20 feet or more in diameter entered by a covered passage opening from outside the platform ; there are irrigation channels and strongly built places perched on hill-tops which ever since MacIver's time have been called ' forts '.

Despite the short time we spent here we were exceptionally fortunate in our finds : Mr Robinson located a very extensive midden in the principal ' fort ', and the pits, which had hitherto yielded nothing to various amateur diggers, provided us with a quantity of beads and information about their construction. We were not a little surprised to find in this inhospitable region beads of the series called by Beck the Zimbabwe Bed Rock Series[9] and by Schofield Series 2[10]; moreover these beads were all in sealed deposits and provided unimpeachable evidence for the age of this culture—17th century according to Schofield but in Beck's view possibly considerably earlier. Apart from a few sherds in the 'fort' we found no ceramic evidence to connect these ruins with those of the Zimbabwe Culture further westwards, but there were a great many sherds directly comparable with ware made by the Manyika tribe who live in these mountains to-day.

The stone-lined pits of Inyanga are commonly called by the European population ' slave pits ' because a popular interpreter of these ruins long ago suggested that the terraces could only have been built by slave labour. The natives have always stuck to the story that cattle were kept here and we see no reason to disbelieve them, with the proviso that the ' cattle ' might possibly have been pigs, goats or sheep as well as very small oxen (the average size of the entrance passages is only 4 feet high by 2 feet wide). Hut floors and the position of refuse show that huts were on the top of the platform surrounding the pit—indeed Arkell's photograph of the pigsty house in the Sudan in a recent number of ANTIQUITY[11] is in close accord with our idea of an Inyanga stone-lined pit in its prime.

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[8] Peters, C., *The Eldorado of the Ancients*. London, 1902.

[9] Caton-Thompson, G., *The Zimbabwe Culture*. Oxford, 1931.

[10] Schofield, J. F., ' A preliminary study of the prehistoric beads of the Northern Transvaal and Natal ', *Trans. Roy. Soc. S. Africa*, XXVI, 341–72.

In order to draw a distinction between the various Ruin Cultures or sub-cultures at Inyanga I have suggested *Niekerk* for that of the van Niekerk Ruins and the allied culture further north, and *Inyanga Upland* is suggested for that in the Upland Zone of Inyanga proper. Before much work had been done at Inyanga I suggested lumping all the Inyanga Ruin cultures together as Iron Age C[12]; two full seasons' work does not shake my belief that here in the north east of Southern Rhodesia we have a stone building culture differing in many respects from that of Zimbabwe in the south west of the Colony, although as has been noted previously there is evidence for some connection between the two cultures.

Perhaps the most interesting historical result of our work is the identity of the Inyanga Upland culture with the modern Manyika tribe; there can be no doubt that these are the people of Manhiqua referred to by the Portuguese in the 16th century as having much gold in their land[13]. Nearly every tribe and most dynastic names in this part of the country can be identified from the 16th century Portuguese records, and our finds tend to prove, as has already been suggested by a former Native Commissioner, Mr W. H. Stead, that in this part of Southern Africa there has been no appreciable tribal movement for the last four or five centuries[14].

Much more remains to be done but our work is beginning to give substance to the hitherto shadowy 'Empire of Monomotapa'.

Although the ruins took most of our time Mr Robinson managed to undertake three digs in rock shelters and to make important surface collections of stone implements. These bid fair to compel the amendment of some of our views on the Stone Age in Southern Rhodesia, which is at present known only from sites in the west of the Colony.

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[11] Arkell, A. J., 'The Pigsty House in the Nuba Mountain', ANTIQUITY, XXIV, 101.

[12] Summers, R., 'Iron Age Cultures in Southern Rhodesia', *S. Afr. Jnl. Sci.*, XLVI, 95–107.

[13] Axelson, E., *South East Africa, 1488–1530*. London, 1940. (Penhalonga, in the heart of the Manyika country is a rich gold area to-day and on the other side of the border alluvial gold is still worked by this same tribe).

[14] Stead, W. H., 'The People of Early Rhodesia', *Trans. Rhodesia Sci. Assn.*, XLII, 75–83.

Sutton Hoo—a rejoinder

by R. L. S. BRUCE-MITFORD

With a note on the coins by JOHN ALLAN

DR GORDON WARD[1] says that my paper 'Sutton Hoo—Recent Theories'[2] is described by me as 'embodying the official views about Sutton Hoo' and 'thus claims very particular authority'. What I said in fact was (op. cit., p. 1) that, since opposite opinions had recently been expressed on certain critical aspects of the find (Pagan or Christian ? Grave or Cenotaph ? English or Swedish ?), 'it is desirable that an official assessment of the issues should be offered to students, even though it cannot be regarded as final'. Anyone who can get into print can say what he likes about Sutton Hoo. Those officially engaged on the study and definition of the material naturally bear a greater responsibility for their utterances. They should also be acquainted with the facts, which not all commentators have been. By 'official assessment' I meant no more than that, as a sympathetic reader must at once have seen.

There is no point in arguing about the spoons, or other factors, as favouring Redwald as the man for whom the monument was raised, if the coin evidence indisputably shows that it is too late to be Redwald's. Let us then turn straight to the coins. I fully agree with Mr Crawford that 'there still has been no full and frank statement of the evidence', nor will the present 'rejoinder' supply that deficiency. I had, however, taken pains to look critically into this all-important factor, *viz.* the evidence of the coins for the date of the burial, before committing myself to the long article quoted above, and can bring forward three recent and hitherto unpublished numismatic opinions. Two of these were available to me when I wrote the article referred to, the third has since been added by Mr Philip Grierson.

First, however, I must say that Dr Gordon Ward's remarks on the coins are not helpful. He only serves up the familiar comments of 1939–40, which I personally have read and digested many times in the attempt to get to the bottom of this vexatious and all-important problem. When he fails to make any mention of Dr John Allan's post-war verdict and says (p. 13) 'no single numismatist has come forward to say that the burial cannot date between 600 and 640', it is clear that he has either forgotten, or not read, the *British Museum's Provisional Guide* (1947). He also overlooks specific statements on p. 26 of my paper, and on p. 23, e.g. '650, the earliest date considered on numismatic opinion to be possible for the Sutton Hoo ship-burial'. He ought not to assume that I made this up. What are in fact the numismatists' opinions, not of 1939, but of 1950 and 1951 ? The first account which I can cite is that of Dr John Allan, formerly Keeper of Coins and Medals in the British Museum. It was sent to me in 1951 and supersedes the opinions which he expressed in his lecture to the Society of Antiquaries in January 1946, and which were utilized in the Museum's *Provisional*

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[1] ANTIQUITY, XXVI, 1952, 9.

[2] *Proceedings of the Suffolk Institute of Archaeology and Natural History*, vol. 25, part I, 1949, published 1950, 1–78.

Guide (p. 42). A detailed description of the Sutton Hoo coins by Dr Allan is expected shortly in the *Numismatic Chronicle.* His interim report runs as follows :

* * * *

'The Sutton Hoo Treasure contained 37 gold coins, 3 similar coins obliterated preparatory to melting down, and two small gold ingots. The coins are all of the Merovingian period of France. The ingots are of the same period, as we know that the Merovingian tax-collectors had the coins paid to them melted down, refined, and paid into the Treasury in ingots of pure gold. The Merovingian coinage began in the 6th century by copying the contemporary Byzantine coinage of Justinian, Justin II, and especially Maurice Tiberius (582–602) ; a clue to the chronology of the coins is given by the gradual corruptions of the types and legends and in particular the change of the reverse type from Victory to cross on globe or steps which took place towards the end of the 6th century. The Merovingian kings issued coins in their own name from the time of Theodebert I (534–48), but by the end of the 6th century the coinage of moneyers had begun which in the 7th formed the overwhelming proportion of the issues. These moneyers struck coins bearing a rude portrait of the king, but the legend consisted of the moneyer's name and place of issue. Of the latter hundreds are known from all over the country.

'More attention has been paid by students to the identification of the mints than to the chronological arrangement of the coinage. The broad outline of the latter is known from the evolution and degeneration of the portraiture and the gradual change in reverse type from Victory to cross, to cross with XP symbol, cross with omega, anchor-cross to plain cross. We have a few aids to date styles, as for example from the coins of the Church of St. Pierre de Corbie, which we know cannot be earlier than 657 when it was founded.

'All the coins from Sutton Hoo are tremisses (thirds of solidi). The following mints are represented :—Andernach, Angers, Bordeaux, Chambly, Châtre, Dinant, Gevaudan, Huy, Laon, La Trémouille, Marseilles, Metz, Mouzon, Montignac, Paris, Rodez, Sion, Sens, Troyes, Valence, Usson and Uzès.

'The find contains no coins with reverse type Victory, so that one can say roughly that there are no coins of the 6th century in it. The absence of solidi points in the same direction, for they went out of fashion in the 7th century. The only coin bearing a king's name is one of Theodebert II (595–612), an unpublished piece and apparently the second known coin of this ruler. The cross on globe reverse makes an attribution to Theodebert I impossible. Confirmation of the attribution is found in its resemblance to a coin of the moneyer Manileobo of Mouzon, who is the moneyer (at Clermont Ferrand) of the known coin of Theodebert II. The earliest coin is the imitation of Justinian of Uzès which still bears the cross potent and might even be of the end of the 6th century. The three coins with traces of the legend of Maurice Tiberius are of the early 7th century, and most of the coins of good workmanship with cross-on-globe or cross-on-steps reverse belong to the first quarter of the century. One certain date in the Merovingian coinage is given by the moneyer Eligius (who is known to be St. Eloi who died in 659) who worked for Chlotar II (584–629), Dagobert I (629–39) and Clovis II (639–57). There are no coins in the find bearing Eligius' name, but a number have types introduced by him or are later imitations of his types. To his period belong the coins of Paris with R on the cross, and the coins with omega on the cross which he introduced in the reign of Dagobert I, while the coin of Bordeaux has a portrait based on that of Clovis II.

'In the provincial mints and later issues which copied the coinage of Eligius the omega on the cross was misunderstood or forgotten and became a curve which from its resemblance to an anchor is called by French numismatists the "croix ancrée." The coins of Laon, Châtre and Chambly, for example, must be put after the direct influence of Eligius had gone, i.e. later than 660. Later still is the coin of Usson (the portrait on which is approaching in style that of Childebert III) with a reverse arrangement which became regular in the 8th century and is only found on coins known to be late, e.g. at St. Pierre de Corbie. The busts on the coins of Chambly and Montignac must be put long after Clovis II on whom they are modelled—indeed, French numismatists put such barbarous pieces towards the end of the 7th century. The two coins of Gevaudan are clearly very late when one compares them with the regular issues of this mint. The bust on the coin of Rodez is likewise later, while the small pieces with barbarous types cannot be contemporary with any coins of good style. It seems impossible on grounds of style and type to put the date of composition of the hoard before 675.

'Such comparison as is possible with other hoards points in the same direction. The Crondall hoard is dated 660–70; it was similar in constitution to Sutton Hoo but contained one coin earlier than any in the latter, while the latter has a number of barbarous pieces later than any at Crondall.

'The great Bordeaux hoard had no coins as early as some in Sutton Hoo, but the general run of types was the same and several coins were common to both hoards, as for example that of Usson, which we already believe to be late; it contains a number of coins of Gevaudan, but none as barbarous as in Sutton Hoo. It contains a number of Visigothic coins made from 576 to 680, and its date of burial is therefore known to be not before the reign of Wamba, A.D. 672–80.

'Little comparison can be made with the Wieuwerd find, which is also rich in jewellery. The latest datable coins in it were of Heraclius (d. 640); it contained a large proportion of solidi and coins of Chlotar II (584–629). A comparison would suggest that the Sutton Hoo hoard, which contained no solidi and none of the comparatively common coins of Chlotar II, is later than the middle of the century, when the Wieuwerd hoard may have been buried.

'The evidence then suggests a date not earlier than 675 for the composition of the hoard. It should be remembered, however, that this is not an ordinary hoard buried in a time of unrest and reflecting the currency at the moment of burial. It does not represent the currency of England at the time nor is it money brought back from a trip to the continent. The presence of the ingots and defaced coins in it which would not get into ordinary currency suggests it was originally in a moneyer's or tax collector's office (they were closely connected under the Merovingians) and looted from there and brought to England. How long before this took place before the coins came to be buried with the owner's other heirlooms it is impossible to conjecture'.

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Dr Allan thus places the *burial* of the hoard (i.e. the date of the grave) some time *after* A.D. 675; in correspondence with the writer he suggests the date *c.* 680.

We may now turn to the second numismatist's opinion, that of M. Jean Lafaurie, M. Le Gentilhomme's successor at the Cabinet des Médailles (where the great bulk of comparative material is concentrated) and one of the most distinguished younger French numismatists. Photographs and (by courtesy of the Keeper of Coins and Medals) a set of casts of the Sutton Hoo coins was sent to him with a brief *questionnaire.* The writer also had the opportunity of discussing the coins with him personally in Paris. The relevant part of his final reply is as follows :—

> ' Je viens d'examiner à nouveau les photographies et les moulages des monnaies composant la trouvaille. J'ai essayé à nouveau quelques identifications en particulier pour les triens no. 24 a 37, sans succès d'ailleurs.[3] Quant à la date d'enfouissement il n'est guère possible de la placer antérieurement à 650 et certains types sont assez tardifs ; par exemple le triens no. 32 de la photographie, d'un atelier de la région parisienne indeterminé, porte " l' appendice perlé " détail que n'apparait qu'en 650±[4] ; les no. 28, 33, 34, 35, 36, 37 sont encore plus tardifs et doivent être en or pâle. Le numéro 10 de St. Étienne de Bordeaux date de la période 650–75, période assez tardive, sans doute aussi vers 650. Un exemplaire de cette pièce faisait partie de la trouvaille de Bordeaux etudiée par Pierre Le Gentilhomme ; la date d enfouissement de ce trésor était très bien daté de 675–80. Peut-être votre trouvaille est-elle un peu antérieure (660–70), mais il n'est *pas possible* de la dater de 625. L'absence de deniers d'argent et de sceattas anglo-saxons la place bien antérieurement à 700 '.

It may be remarked here that the Sutton Hoo hoard in fact contains no coins of ' pale gold ', in the sense of obviously base metal. Some of the coins are paler than others, but these include some of the pseudo-imperials, i.e. the earliest coins in the hoard. No evidence of chronological value can be derived from colour-distinctions within the hoard.[5]

M. Lafaurie thus says that it is ' hardly possible to put the date of the burial before 650 ' and suggests that some of the coins indicate a later date. Perhaps the date of burial, he says, may be a little before that of the Bordeaux hoard (675–80), that is to say it may be 660–70, but ' it is *not possible* to date it to 625 '.

Now what are these numismatic opinions worth ? There is a natural enough tendency amongst laymen to swallow numismatic verdicts whole, and to place more weight than they deserve upon coins as a dating factor in fields of numismatic study which are in fact not yet, and may never be, susceptible to precise datings. Sceptics in 1939 and 1940, in the persons of Mr Crawford and Mr Dayrell Reed, did a service in pointing out—and rubbing in—the uncertain nature of the numismatic evidence in this instance as an instrument of firm and precise dating. But there is now a tendency to go too far in the opposite direction and wave the evidence of the coins aside. The broad picture derived from the 37 Sutton Hoo coins, without laying undue stress on any one, is really not to be set aside. In his observations in an accompanying article, Mr Grierson replies to Dr Ward's strictures on Merovingian numismatics in general and to some of his particular points with reference to Sutton Hoo coins, and re-establishes the broad value of the dating criteria available. I must confess, however, that I personally as a layman do not feel convinced by Dr Allan's arguments for the exceptionally late date of 680. Without stopping to consider the historical and archaeological facts which render it highly unlikely, I cannot see the logic of some of his numismatic points, and must raise one or two issues with regard to his commentary.

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[3] *i.e.* beyond those previously identified by Dr Allan as belonging to specific mints.

[4] Its appearance in Suffolk and burial there would of course be later. (R.B.M.)

[5] Dr Plenderleith and Dr Moss, of the British Museum Research Laboratory, have also pointed out to me that the phenomenon of ' surface enrichment ' in antique gold, whereby the surfaces of gold-alloy will turn to pure or nearly pure gold, by the working out from the surfaces, over long periods of time, of other constituents of the alloy, renders deductions from surface appearance or analysis of surface scrapings, extremely precarious. Specific gravity tests are much more reliable as indications of the baseness or otherwise of antique gold coinage.

Dr Allan does not regard the Sutton Hoo purseful as an ordinary hoard buried in a time of unrest and ' reflecting the currency at the moment of burial ' but suggests that the coins came together in a continental moneyer's or tax collector's office on the continent, and that it was looted from there. The distinction is important. A looted hoard might be kept as a memento intact and unmixed with any local currency for some time ; and might later be buried, still intact, either still as a memento of the episode that led to its looting, or simply as treasure. Dr Allan's date of 675 for the composition of the hoard would represent the date of the raid on the continental moneyer's office, not the date of the ultimate burial of the coins in East Anglia, which could be presumed to be some little while later. Again, if the hoard remained intact as a piece of loot, not reflecting the currency at the time of its burial, the absence from it of Anglo-Saxon sceattas or gold coinage need not imply that the burial must pre-date the emergence of the sceatta coinage or lie outside the area of circulation of Anglo-Saxon gold. It need only imply these conditions for the locality and date of the looting, which tells us nothing.

Dr Allan's reason for supposing the Sutton Hoo hoard to have been looted from a continental moneyer's office is that along with continental coins (which alone are present) were the two ingots and what he presumes to be three defaced coins, i.e. coins once struck but hammered blank, perhaps for re-striking. These billets and flans he regards as objects that would only occur in a moneyer's or a tax collector's office.

This does not seem to me to be necessarily, or even probably, correct. It should not be assumed that moneyers or tax collectors were the only people who worked with small gold ingots or hammered out unwanted coins into plain, even discs. Goldsmiths no doubt used such small billets in fashioning the gold minutiae of the period—pins, barrel-beads, thin strips for folding into the cell-walls of cloisonné work, wire and granulation for filigree work, and so on. The Sutton Hoo coins could have been accumulated in Suffolk as bullion or treasure, the blanks and ingots being simply additional bullion, gold in transition from coinage into jewellery. It is in any case not certain that the blanks are hammered-out coins at all ; Mr Grierson has pointed out to me that there were similar blanks in the Crondall hoard, and that they may well be evidence of a habit of locally supplementing a meagre supply of stamped coins by unstamped pieces of metal of the appropriate weight. If the blanks and ingots can be explained in this way, it is no longer necessary to consider the hoard as loot from a continental source. The coins, though foreign, could have been in circulation in East Anglia. Merovingian gold coins were circulating freely in Kent and South England, and it would be very surprising if they were not also in circulation in Suffolk, the connections of which, especially the South-east, with Kent and the continent were at this time close, as the heterogeneous contents of the Sutton Hoo burial itself shows.[6]

There is also something to be said on strictly numismatic grounds against the idea that the hoard was assembled on the continent. Mr Grierson has put the point in correspondence with the writer as follows :—' If it had been collected together on the continent at all, it would show some kind of local concentrations, like the groupings in the Bordeaux hoard, the presence of a number of identical coins of the same local mints in the Mons hoard, and the Quentovic and English groupings in the Crondall hoard. One would expect these groupings even if the hoard had been brought back by an English merchant : there would be some concentration to show where he had traded or spent his time '. Again, the coins were buried in a sumptuous purse of local East Anglian manufacture. True, this may have been used to carry rings, jewels, ingots and other treasure, such as Germanic kings dispensed, but the presence of the coins in a native purse seems rather against the idea that they are loot and in favour of the idea that coins (Merovingian ones, as in Kent) were in circulation. No rings or jewels or other treasure were in fact in the purse, beyond the ingots, which may have been added as a bullion reserve at the time of the burial. The Crondall purseful of coins (101 in number, including one forgery and three blanks, and with no other objects like jewels or rings) tends to reinforce this feeling. Finally, there is no historical evidence, or background, for Anglo-Saxon raids on Merovingian territory at this time.

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[6] See R. L. S. Bruce-Mitford, ' Saxon Rendlesham ', *Proceedings of the Suffolk Institute of Archaeology.*

If Dr Allan's view of the Sutton Hoo hoard as loot is not accepted, we may take the date of burial to be near enough the date of composition of the hoard; and we need not allow for any special time-lag; i.e. we can forget about 680+, and think rather in terms of 675+.

The view taken of the nature of the hoard has some important numismatic implications, as indicated above; but it makes only a little difference to the upper limit of date for the burial. On this point, other arguments of Dr Allan do not seem to me convincing. For example, it does not follow that the coins of Bordeaux, Châtre, Laon and Chambly, which have anchor-cross reverses, 'must be put after the direct influence of Eligius had gone, i.e. later than 660', merely because these reverse designs represent an evolution from a reverse design introduced by him perhaps as early as 628–30. Eligius died in 659, but we cannot assume that his influence remained dominant until the moment of his death. The mints referred to are not far, geographically, from Paris, where Eligius exercised his functions as mint master, but there is no reason to suppose that he exercised such influence over independent provincial mints that they could not develop or vary a design copied from one of his royal issues until he was dead. He did not control the provincial mints. Similarly, even if the busts on the coins of Chambly and Montignac are modelled on the royal bust that appeared on the coins of Clovis II, it does not at all follow that they must date 'long after Clovis II' (639–57). An early coin of Clovis might have reached a remote provincial mint, such as Montignac in the Bordeaux region, within a year or two of its issue, and degenerate or unskilful local copies of it could have been quite easily produced there actually some time before Clovis' death in 657. Indeed, one approaches Dr Allan's late dating of individual coins with the caution which judgments based on style demand, when coins were being struck in a very large number of small towns and villages and when moneyers varied greatly in competence and creative ability, so that good and very degenerate issues could be struck at different mints contemporaneously. It was always possible for a new moneyer to reform the dies of a mint, so that coins in good style might actually post-date and supersede degenerate ones. Such considerations may well explain the presence, in the Crondall hoard, of a few coins which at first sight appear to be later in date than any in Sutton Hoo.

At this point we may bring the third numismatic opinion into the picture. Since Mr Grierson's comments will be found in an accompanying article, it is only necessary to summarise them here. His conclusion is that on purely numismatic grounds the burial cannot be dated before 650, and that there seem to be good reasons for ascribing it to the decade 650–60. This, as we have seen, is a little earlier than the date (660–70) suggested by M. Lafaurie, though the latter does not definitely exclude a slightly earlier date. Mr Grierson observes also that it does not differ materially from that proposed by M. Le Gentilhomme. The latter favoured the decade 640–50, but on the assumption that the hoard had taken shape in Gaul; if, as seems likely, it came together in England, it would be reasonable to allow for a short time-lag. All three solutions would regard 675+ as decidedly too late.

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From the foregoing accounts, though the coins themselves are not yet published some firm conclusions emerge. First, it is quite impossible to identify the grave with Redwald. Secondly, it no longer seems possible to consider any date for the Sutton Hoo burial before 650. Thirdly, numismatic opinion is not agreed in support of an earliest date as late as *c.* 675. The consensus of numismatic opinion points clearly to somewhere between 650–70. There is no numismatic evidence which certainly precludes a date in the decade 650–60, and Mr Grierson actually favours this date, rather than a later one, on numismatic grounds. The crux of the matter lies in Dr Allan's sentence 'More attention has been paid by students to the identification of mints than to the chronological arrangement of the coinage'. The two great standard works on Merovingian numismatics, by Prou and Belfort, appeared at about the same time, the first in 1894, the second between 1892 and 1895. Very considerable original research on the dating of the Merovingian series is needed, with the 37 Sutton Hoo types as the centre of study, before what can be described as a final dating of the burial can be arrived at. This task I am glad to say is being undertaken by Mr Grierson. Meanwhile, a date of burial after *c.* 650 must be accepted, and when to the evidence of the coins, archaeological considerations and the known facts of seventh century East Anglian history are added, as described in the paper in Suffolk Institute Proceedings referred to, it seems to me that this date cannot be long after 650. If so, the identification of the Sutton Hoo monument with one of the three brothers Aethelhere, Anna and Aethelwald seems in the highest degree probable.

With regard to Dr Ward's other arguments, there is no point in mentioning everything with which I disagree. He does not, however, seem to appreciate the difference between evidence and hypothesis. There is no evidence that the spoons belonged to Redwald, though it is quite likely. But even if they did, and the standard also, it in no way follows that the grave they were buried in is his grave. In my article I say that they may have survived in the royal treasury, or been handed on to his son or a nephew. Again, Dr Ward ignores (op. cit., p. 11, item 3) the evidence that there was no body in the burial. Does this fit with Redwald ? His items 3 and 4 simply indicate that he has not taken in my treatment of these subjects. His item 5 I cannot understand at all.

The truth is that there is a quite simple explanation of the size, richness and distinction of the grave. It is the first intact royal inhumation ever found. Of course it is bigger and better than previous graves. But this in no way proves it to be Redwald's. My comparison with the royal cremations at Old Uppsala and with Childeric's grave suggests that its furniture was nothing very out of the ordinary as a royal grave of the era. Nor can I agree with Dr Ward that were it not for the alleged evidence of the coins the grave would be unhesitatingly identified with Redwald. Purely archaeological considerations and particularly the gold jewellery with its affinities with the Book of Durrow, seem to me to suggest a later date than 625–6. These points, however, need not be pressed, since the coin evidence in fact precludes Redwald.

The Dating of the Sutton Hoo Coins

by PHILIP GRIERSON

DR GORDON WARD'S summary of the evidence of the coins and of the views of numismatists regarding the date of Sutton Hoo calls for some comment. Its object is apparently that of demonstrating that, as he himself puts it, 'the numismatists have been set a task for which they are not equipped'. The archaeologists, he suggests, must be left to settle the date between themselves, and the numismatic 'evidence' can simply be ignored. If this 'evidence' runs counter to his belief that the burial commemorates King Redwald, so much the worse for the 'evidence'. It is too uncertain and imprecise to be of much use anyway.

Dr Ward is not entirely to blame for having succumbed to this opinion. It is largely a consequence of some rather hasty remarks on Merovingian coinage in general, and the Sutton Hoo coins in particular, published by Mr Derek Allen in 1939, shortly after the acquisition of the coins by the British Museum. These remarks were subsequently commented on by Mr Dayrell Reed in terms which suggested that the evidence of the coins was so vague as to be valueless. Although the negative views of these two scholars were countered by M. Pierre le Gentilhomme, who had embarked in the thirties on the study of the chronology of Merovingian coinage and had made more progress in this field than all his predecessors combined, he was not able to develop his views at length before his death, and the exact import of those he had expressed was missed by subsequent commentators. The result was that when in 1940 the Editor of ANTIQUITY summed up the various opinions that had up to then been put forward, he felt justified in indulging in the cynical comment that 'where such a range of possibilities exists, a difference of five or ten years cannot be detected, and that is all that separates the numismatists from their archaeological and historical colleagues'.

Mr Derek Allen's distinction as a numismatist is so universally admitted that no harm can be done to his reputation by saying that his article on the Sutton Hoo coins is not his best work. He was suddenly called upon to deal with a number of coins in a series of exceptional difficulty, with which he had no previous familiarity, at a time when men's minds were concentrated on far from academic matters and when contact with foreign scholars and access to foreign collections was no longer possible. The consequence was that he committed himself to three statements of a very misleading character. The first was that in regard to the dating of Merovingian coins, 'unless the king's name is present we have nothing but style to guide us'. The second was that a coin in the Sutton Hoo hoard which he ascribed to Theodebert I (534–48) was a 'later copy of one of his, and may not have been struck till many years after his death'. The third was that 'from the evidence of the coins, the Sutton Hoo burial cannot have taken place before about A.D. 600, is not likely to have taken place earlier than A.D. 640–50, and might even have taken place nearer A.D. 670'.

The second of these points can be disposed of at once, since it is irrelevant to the main issue. It is important only because the suggestion that the coin is a forgery has been subtly used to create a prejudice against the evidence as a whole. In fact, the coin is not one of Theodebert I, but of Theodebert II (595–612). The obverse legend is identical with that of coins of Theodebert I, but this is of no more significance than is, say, the identity of legend on coins of Edward III and Edward I; it is a consequence of the fact that both sovereigns had the same name. The lettering of the coin is quite characteristically that of coins of the moneyer Manileobo, who minted for Theodebert II at Clermont-Ferrand, and who clearly revived an old legend for his master. Although the coin has frequently figured in subsequent discussions of the date of the hoard, it has no relevance at all in this connection.

Much more important is Mr Allen's statement that style is the *only* criterion available for dating non-royal Merovingian coins, for the usefulness of style in this connection was strongly criticized by Mr Reed. He contended, in the first place, that it was much too vague. There might be an error of as much as thirty years in either direction, and if the coins corresponded fairly closely—Mr Allen had noted that they did—with those of the time of Clovis II (639–57) this would mean no more than that 'they might have been struck at any time between 615 and 675'. In any case, Mr Reed argued, the large number of mints involved must to a considerable extent invalidate stylistic arguments altogether. The competence of the workmen employed in different mints would be too disparate for one to be able usefully to compare their products.

The second of these considerations has considerable truth behind it, though with the necessary proviso that however early a date we may suggest for an imitation, no amount of reasoning can make an imitation earlier than its archetype. In view of the generous latitude in chronology allowed by Mr Allen and Mr Reed, this proviso is a very relevant one. The hoard *cannot* be as early as 600 (Mr Allen), or even 615 (Mr Reed), for the archetype of the 'anchor-cross' series, of which there are seven derivatives in the hoard, did not antedate the 620's, and the 'chalice' series of Banassac, two degenerate specimens of which occur in the hoard, only appeared, so far as our knowledge goes, under Charibert II (629–32).

The imprecision of stylistic evidence is also greatly exaggerated by Mr Reed. It must be remembered that in comparing one hoard with another of a different date, the *average* composition is bound to be different, and stylistic criteria are here both relevant and reliable. One would expect a hoard dating from between 615 and 639—for such dates Mr Reed would be prepared to allow as possibilities—to contain a fair proportion of the 'Victory' types of the sixth century and a considerable number of 'Maurice Tiberius' coins, to say nothing of stray Byzantine pieces. By the middle of the seventh century these would have almost, if not quite, disappeared. If the average composition of the Sutton Hoo hoard corresponds (as it does) to the general run of coinage current in the time of Clovis II, we need have no hesitation in ascribing it to the period of this monarch's reign.

The real fallacy of Mr Reed's argument, however, lies in the fact that he has taken too literally Mr Allen's statement that the chronological classification of the non-royal Merovingian coins can only be determined by considerations of style. There are other methods to be employed as well. There is the evidence of a few hoards which, owing to the presence of Byzantine or Visigothic coins in them, can be dated with precision, and which, like the names of sovereigns on the 'royal' series, give the numismatists certain fixed points from which to work. There is also the evidence of die relationships, and that provided by the work of individual die-sinkers who were employed in more than one mint.

For example, Sutton Hoo contained a coin of Usson struck by the moneyer Disiderius. There are two other coins of the same moneyer in the Cabinet des Médailles in Paris. All three have different reverse types. A comparison of the Sutton Hoo coin with one of the Paris specimens shows that they have a common obverse die, which was much fresher when it was used for the Sutton Hoo coin than it was when used for the Paris coin. The Sutton Hoo reverse must therefore precede the Paris one in date. The obverse of the third Paris coin is copied, not very skilfully, from the obverse we have just been considering; the reverse of this coin must therefore be later than the two other reverses. Here, quite simply, a study of the dies has allowed us to place three reverse types in their correct chronological order, and it is not irrelevant to note that it was the third and latest of these coins that was included in the Bordeaux Hoard of *c.* 680.

Again, the coin of Huy in the hoard links up through its moneyer Landigisilus with a die-sinker who made dies for Betto at Soissons, Theudegisilus at Metz, Arnulf at Strasbourg, and other moneyers elsewhere. For some of these mints we have not sufficient material to allow us to establish a clear chronology. But for the more prolific mints this is a quite feasible task, and the interlocking of types and the activities of individual die-sinkers can be used in many cases to bring the minor mints into the picture. Even where relatively unimportant series are concerned, we can in this fashion arrive at something much more definite than the 'not earlier than 600, not likely before 640–50, and possibly as late as 670' of Mr Allen, and the 'any time between 615 and 675' of Mr Reed.

The evidence on this point is as a whole quite clear, even though precise dates cannot be given for individual coins. The majority of the coins in the hoard belong to the second quarter of the 7th century. The anchor-cross type already alluded to, for example, was inaugurated at Paris during the reign of Chlotar II (613–29), and started as a cross with an omega above and an alpha below. This was quickly simplified by the omission of the alpha and the fusion of the omega in varying degrees with the cross. In the 630's and 640's it became one of the commonest types over a considerable area of west-central France. There are seven specimens in the hoard, and they can certainly be ascribed to the second quarter of the century. One of the coins of this type has the bust described as 'à l'appendice perlé', the origins of which are dated by French numismatists as *c.* 640–5.

M. Le Gentilhomme stated all this quite clearly in 1940, and only special pleading of the most determined kind has enabled later scholars to discount what he said. 'L'activité de ces derniers ateliers'—he is referring to such rural mints as La Trémouille, Usson and Montignac in Poitou and the Limousin—'et la présence de la croix dite ancrée sur les monnaies de Saint-Etienne de Bordeaux, de Laon, de la Trémouille, du fisc et sur quelques pièces indeterminées, [nous] obligent à descendre à l'époque de Clovis II, caracterisée justement par l'éparpillement des ateliers ruraux et la faveur de ce type de croix'. The reign of Clovis II means 639–57; it does not mean a decade or two earlier.

This, however, is only the earliest possible date at which the burial could have taken place. With regard to the most likely date, some hesitation is possible. M. Le Gentilhomme favoured the first half of the reign of Clovis II—i.e. the 640's—on the ground that the 'anchor-cross' type, though important as an element in the hoard, was not yet the predominant one on coins coming from Neustria and Aquitaine. To this view there are two objections. One is that while the argument would be valid if the hoard had been put together in France, it loses its cogency if the hoard had come together in England, as there are good reasons for supposing it to have done; a time-lag between the striking of the coins and their crossing the Channel would explain why the 'anchor-cross' types do not yet predominate. A more important consideration is that while there is no coin in the hoard that can be ascribed with certainty to a date posterior to 650, there are several that must have been struck about that time.

On the other hand, a date of burial appreciably later than 650 is rendered unlikely by the presence of as many as six coins—a partial imitation of Justinian (527–65), three imitations of Maurice Tiberius (582–602), a coin of Theodebert II (595–612) and another of his moneyer Manileobo—which can be ascribed with certainty to the end of the 6th or the opening years of the 7th century. These coins are all different from one another, and are certainly not a secondary hoard of the type which, when incorporated bodily in another hoard at a later time, can lead the student far astray in matters of dating.

A provisional conclusion is that the hoard cannot be earlier than 650, since it contains a number of coins which any numismatist must ascribe to the 640's. On the other hand, it is unlikely to have been buried at any date later than 660, both for the reason just given and because it contains no obviously late types or any specimens of the debased gold which became normal towards the end of the century.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

PREHISTORIC EUROPE: the Economic Basis, by J. G. D. CLARK, with 16 plates and 180 text illustrations. Methuen, 1952, £3. [A major contribution to the subject which will be reviewed in due course].

MEMORIAL VOLUME TO SIR ALFRED CLAPHAM: *Arch. Journ.* vol. CVI, supplement. R. Arch. Inst., 37 Onslow Gardens, S.W. 7, 12*s* 6*d* to non-members [Essays by his friends chiefly on architecture and all of a high order of scholarship].

BEGINNING IN ARCHAEOLOGY, by KATHLEEN M. KENYON. Phoenix House, London; 12*s* 6*d*. [An authoritative book for beginners].

BOATS AND BOATMEN, by T. C. LETHBRIDGE: Thames and Hudson. 15*s*. [No armchair stuff and, like all he writes, most eminently readable: very strongly recommended].

THE HERITAGE OF EARLY BRITAIN, by various authors. G. Bell, 12*s*. [Yet another excellent book of a popular kind].

DEVONSHIRE STUDIES, by W. G. HOSKINS and H. P. R. FINBERG. J. Cape, £1 16*s*.

CASTLE DORE. Report on the excavations at, by C. A. RALEGH RADFORD. *Journ. R. Inst., Cornwall*, N.S. vol. I, 1951.

DISCOVERING THE PAST, by JACQUETTA HAWKES. Nat. Council of Social Service, 26 Bedford Square, London, W.C. 1, 2*s*. [A popular handbook explaining the techniques and raw materials of archaeology for beginners, so far as this is possible in 15 pages. It should have been longer].

ARCHAEOLOGICAL SURVEY IN THE LOWER MISSISSIPPI VALLEY, by P. PHILLIPS, J. A. FORD and J. GRIFFIN. *Peabody Museum Papers*, vol. XXV, 1951.

Notes and News

CELTIC CHARIOTS ON ROMAN COINS

The purpose of this note is to draw attention to representations of Celtic chariots on Roman coins which have not hitherto been adequately discussed or illustrated. Sir Cyril Fox, in his Anniversary Address to the Society of Antiquaries in 1947, reproduced an enlargement of Déchelette's illustration of a coin of the Remi which represents a chariot of Celtic type with semi-circular side-screens[1], but there are other representations which, since Beale Post drew attention to them in 1853[2], have not been discussed from the point of view of the archaeology of the vehicles represented upon them.

The coins are illustrated by enlarged photographs in PLATE I. The first (top left) is a denarius signed by Scaurus[3] and usually attributed to 92 B.C., but Professor Michael Grant points out to me that a case has been made out for ascribing it to 118 B.C. and the foundation of the *colonia* Narbo : a date to which he himself inclines[4]. At all events the attribution of the warrior on the chariot as Bituitus, ruler of the Arverni, seems likely enough. He is shown naked, and holding spear, long shield and *carnyx*. The two-horse chariot has no side-screens but appears to have an open platform with perhaps some sort of wrapping obscuring the feet of the warrior.

The second coin (top right) is of the moneyer L. Hostilius Saserna[5], of the last 50's or first 40's B.C.—a date of *c.* 48 B.C. has been suggested. Here the chariot has the semi-circular side-screens of the Remic type, with a naked charioteer crouching well forward, the reins in his right hand and a whip in his left, while behind him stands a naked warrior with dishevelled hair facing to the rear and carrying spear and long shield. This figure and the head on the obverse have sometimes been attributed to Vercingetorix.

The third coin is one of Julius Caesar[6], and Professor Grant thinks *c.* 49 B.C. a likely date ; although it has been assigned to a Spanish mint, he does not feel that such an attribution is altogether secure. A trophy is represented, arranged on a tree-trunk, by which is propped a chariot, pole-tip against the trunk, with a pair of side-screens of the same semi-circular type as those already noted.

Now while these coins provide welcome iconographic data on chariots which must be Celtic, they (and the Remic coin published by Déchelette) raise chronological problems of some interest. Literary evidence is consistent in showing that the Celtic tribes waged chariot warfare in Italy from the Battle of Sentinum to that of Clastidium (295-222 B.C.) and again in the Delphi raid of 279 B.C., while in Transalpine Gaul Bituitus is specifically described as having a silver-fitted chariot—*argenteo carpento*

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[1] *Antiq. Journ.*, XXVII (1947), 117–19, with Pl. XVIIIa, from Déchelette, *Manuel* II, 1190, fig. 504.

[2] *Journ. Brit. Arch. Assoc.*, VI (1850), 252–5.

[3] *B.M. Cat. Coins Roman Repub.*, I, p. 184, no. 1185.

[4] *Num. Chron.*, 1924, 31 ff. ; 1941, 120 ff. I am most grateful to my colleagues, Professor Michael Grant and Dr John Allan, for their help in this matter. Dr Allan kindly provided the casts from which my illustrations were made, while still in the British Museum.

[5] *BMC Repub.*, I, p. 513, no. 3994.

[6] Ibid, II, p. 363, no. 70.

qualis pugnaverat—in 121 B.C.[7] We may therefore accept the denarius of Scaurus as likely to be a contemporary representation of the Gaulish warrior and chariot. But by 58 B.C. the total absence of any reference to chariot warfare by Caesar, and his surprised recognition of its survival in Britain a few years later, shows that by that date it had been dropped from tactical use by the Gauls.

It is therefore difficult to accept the Hostilius Saserna coin and that of Julius Caesar, both within a year or so of 50 B.C., as representations of contemporary Gaulish chariotry or the reverse of the former as depicting Vercingetorix, and this also applies to the coin of the Remi, again of mid-first century date. Dr Allan, commenting on this coin[8], points out that whereas a chariot could have been copied from a Roman denarius of any date from 150 B.C. onwards, the Remic representation is specifically that of a definite chariot type, known to be Celtic and non-Roman, and not merely a vague approximation to a two-wheeled vehicle with its horses.

We seem left with the possibilities that either the three representations of the chariot with semi-circular side-screens are deliberate archaisms, or, at least with the Caesar and Hostilius Saserna coins, that they represent British vehicles and are not unconnected with the campaigns of 55 and 54 B.C.[9]. It is at this point that the problem must be handed by the archaeologists to the numismatists. STUART PIGGOTT.

A CIRENCESTER TILE

In 1923 an inscribed, clay-baked, roofing tile, found in Cirencester, was placed in the old Bathurst collection of local antiquities, and is now in the new Corinium Museum.

The inscription on the tile has been variously dealt with. For convenience these readings are numbered.

(1) Mr St. Clair Baddeley and Mr Collingwood deciphered it (*Journal of Roman Studies*, no. XI, p. 239) as CLLTXVIIII, explaining this as,

Cl(audius ? and) L(ucius ?) (made 19 T(iles).

The sense of this interpretation—two workers, so little work—left the field open for further inquiry.

(2) A suggestion of my own, published in a local paper at the time, was that the inscription might be read as CETUS 4, implying that a Romano-British tilemaker Whale made four batches of tiles ; how many per batch is not known. The cursive 's' is scratched, as I conceived it, across the stem of the T. In this (possessive) connection, a nominative is found as well as the commoner genitive. Further, to take the ligated T as standing for tegulae (tiles) seems unlikely ; one could see the articles were tiles.

I submitted this attempt to Mr H. B. Walters, Roman pottery expert, in the Greek and Roman Antiquities Department, British Museum, who replied, 'The inscription of which you enclose a reproduction appears to be in the ordinary Roman cursive

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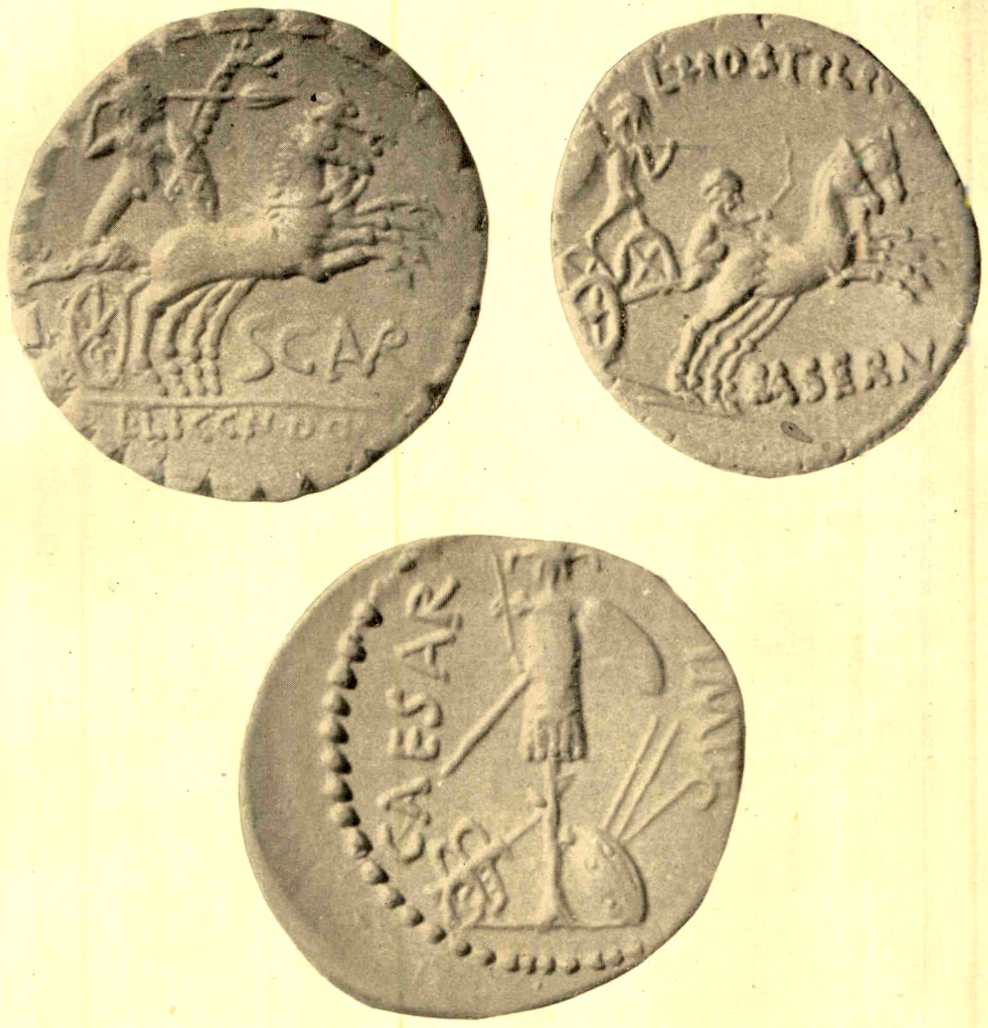
[7] The literary evidence is conveniently set out by Arbois de Jubainville in *Rév. Celtique*, IX (1888), 387–93 and Theodore Reinach in *ibid*, X (1889), 122–33. De Jubainville gives the Bituitus reference, from Florus.

[8] In Fox, *loc. cit.*, 118 n.

[9] Professor Grant notes that it has been suggested by Babelon that the 'Vercingetorix' representation may in fact be a personification of Pallor or Pavor (*Monn. Rep.* I, 549–52), and that an alternative explanation of the reverse would be a reference to a battle fought by Hostilius' legendary ancestor, Tullus Hostilius, against the people of Veii (*ibid*, I, 549). The head may be nothing more than a typified Gaul. Babelon further referred to a British *essedum* when commenting upon the trophy on the coin of Julius Caesar (*ibid*, II, 13).

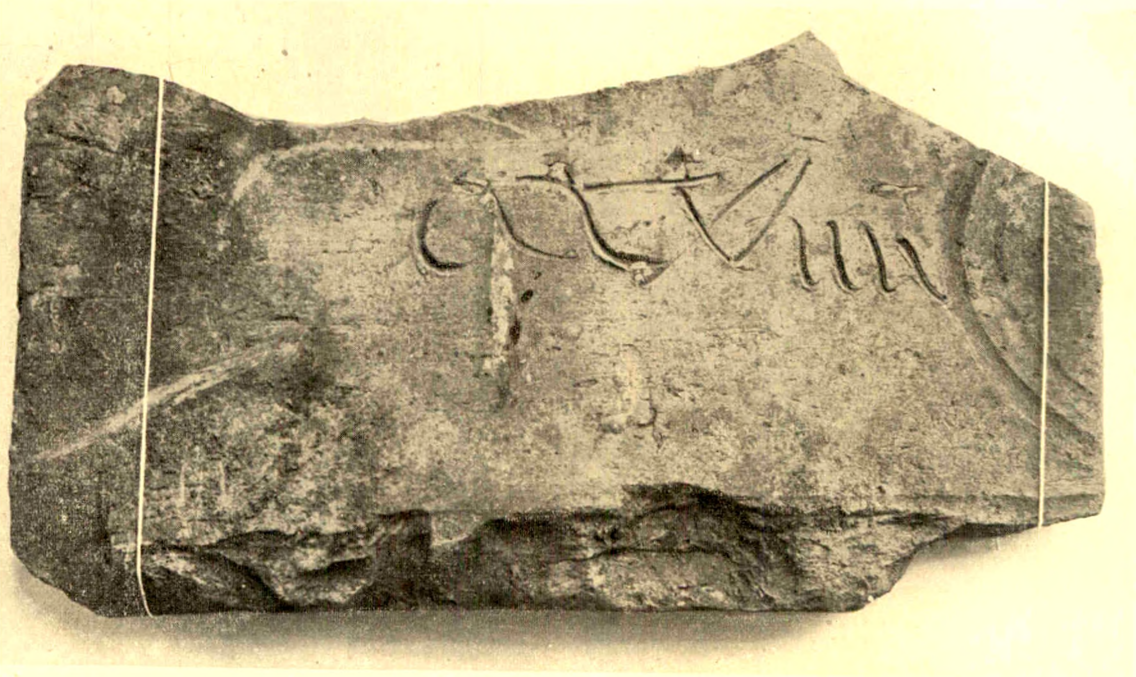
PLATE I



ROMAN COINS, LATE SECOND AND FIRST CENTURY B.C., SHOWING CELTIC CHARIOTS (× *c.* 3)
Ph. P. Glasier

facing p. 88

PLATE II



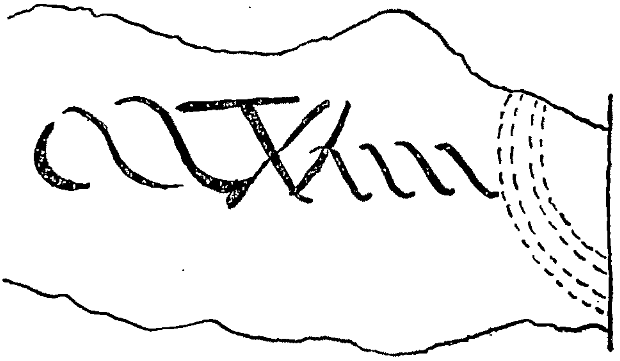
TEGULA DUROCORNOVIANA
Ph. Moss, Cirencester

characters, and your reading CETU(S) is certainly quite possible'. With regard to the reading quoted above, he commented, 'I should be quite prepared to accept it as far as the TXVIIII—it would account more satisfactorily for the ligated letters—but I cannot accept the CLL without considerable hesitation. The characters certainly seem to me to me to be CE'.

(3) Pursuing the matter I wrote to Dr van Hoesen of Princeton University, author of a manual on Roman cursive writing, which he kindly lent me. He explained that his duties had kept him out of touch with palaeography for several years, and he would only risk 'a hasty, off-hand suggestion'. This was that the inscription might be read CCCXXIIII tiles, 324.

With due deference, I do not think this can be supported: if the first stroke is 'C', the next two cannot be; and conversely.

(4) I next laid the problem before Prof. Cumont, the eminent Belgian authority. He would not venture an opinion, but forwarded my letter to an expert here, his friend Prof. Anderson, who replied regretting that neither M. Cumont nor himself could throw any light on the point. He remarked cheerfully that when the tile inscriptions were deciphered not much was gained.



(5) A lady specialist whose opinion I next sought, replied, 'The more I look at the tile the more I am inclined to begin with the ominous letters BL'. Her solution for what it was worth, she suggested, would be 19 bloody tiles.

(6) On a visit to Corinium Museum in 1950, and making as usual for the old tile, I discovered it with the label attached:—'ARTUCI; in cursive script, a potter's name. Deciphered by Felix Oswald'. As we have to do with a tileyard scribble, and not a potter's stamp, this was patently wrong apart from obvious mis-reading of the two end letters.

Thoroughly to clear the matter up I wrote to Dr Felix Oswald who replied:—'I have no record of it, and I fear this is an error, and some one else must have deciphered it and erroneously labelled it as by me'.

(7) That disposed of, I was still unable to let the quest drop which has pleasantly haunted me for more than 20 years. An enquiry at the British Museum's Dept. of Brit. and Med. Antiqq. in 1950 resulted in the following:—

'We think it is a numeral CCCXVIIII, or 319. It was probably scribbled on the last tile of a batch to show the total number in the batch'. Signed: J. W. Brailsford, Assist.-Keeper.

The same adverse criticism holds equally here as in No. 3. It is noticeable however that Nos. 7 and 3 correspond closely.

(8) Visiting the Museum again in Sept., 1951, I found yet another attempt on the graffito, viz., ALTUM. Its author, Mr R. P. Wright, M.A., F.S.A., remarks, 'the upper part of the tile has been broken away, so we do not know what thing was "high"'. (See *Journal of Roman Studies*, No. XLI, 1951).

A serious fault in this case is the overlooking of the definite cross-stroke through the T-stem, and the assumption of lost letters on the broken-off part of the tile seems unwarranted. Further, the last 4 and smaller strokes (if M) seem not to form part of one and the same word.

The Cirencester tile guards its secret despite the exceeding clearness of its lettering: even the order of the strokes as traced in the wet clay can be recovered from the complicated TXV part of the inscription. The trouble is we are not sufficiently versed in Roman cursive to pronounce definitely, otherwise Cumont would not have fought shy of the problem. But this much is certain, whether the scribe was foreman or workhand, whether he used style or twig—his name was not ARTUCUS. J. B. JONES.

EXCAVATIONS AT BODRIFTY, MULFRA HILL, CORNWALL

Bodrifty settlement lies 4 miles north of Penzance, on the western slopes of Mulfra Hill. The site is at the head of the valley reaching from Porthmeor, on the north coast, to the high moors. The area is very rich in prehistoric remains and a ridgeway touches the site.

The settlement consists of many huts, lyncheted fields and enclosures. There is also a ring wall,[1] much mutilated, enclosing about 4 acres of the site, including the area being excavated; much of this is in a furzy croft.

The largest hut circle examined (25 feet across) appears to be of the Iron Age, but the site was probably occupied by an earlier dwelling of the Late Bronze Age. Excavation has shewn that the site of the hut was cleared and levelled by cutting back into the hillside and spreading the excavated rab over the lower part of the floor. Finds occurred in this redeposited rab. Walling set into this make-up would be likely to collapse and this has in fact happened in certain places. The wall of local coarse granite is very wide (8 feet); it is much robbed, but appears to consist of a normal wall of about 4 feet, faced on both sides and backed by another wall, also of about 4 feet, but faced on the outer side only.[2] Most of the stones are laid on their long sides, an Iron Age feature locally; there is no mortar and no real coursing, except on the north side of the five-foot wide entrance. This coursing is unlike the walling found in the other huts examined or that of the huts at Trewey-Foage, a Late Bronze Age site near Zennor.[3] The outer wall appears to be a later addition for a small hearth with a number of sherds was found in the space between. It was probably built to keep out the wet. The hut is very damp and a deep water channel runs along the upper side, where the wall is thickest; between the kerbs of the channel and the wall face is a 'duck walk' of flat paving slabs.[4]

The roof of the hut was supported by a ring of posts, set in holes cut into the floor. These are six in number and average 13 inches in diameter and 12 in depth; they are set

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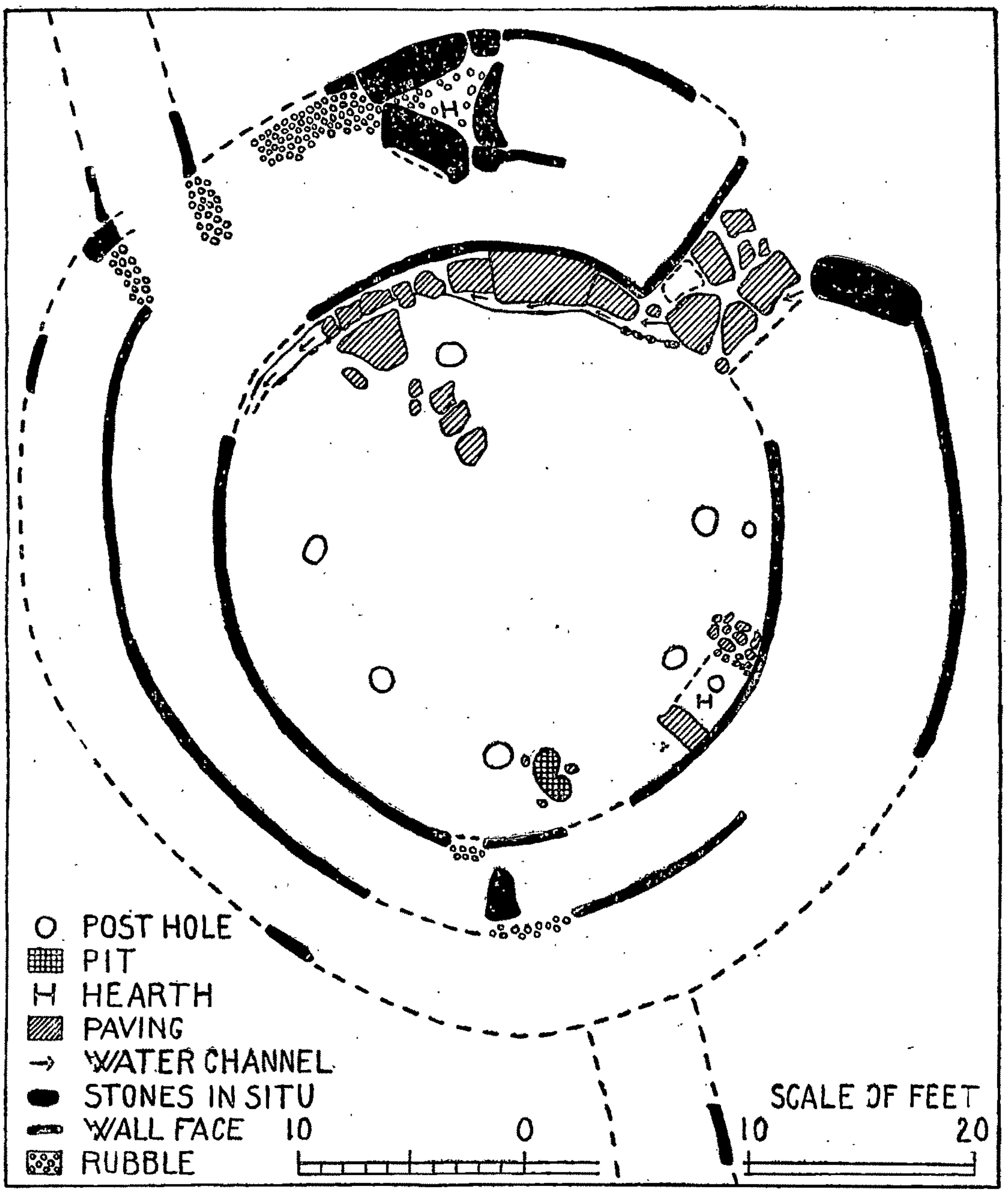
[1] It is hoped to investigate this next season.

[2] Much spoiled by stone robbers.

[3] *Arch. Journ.*, XCVIII, 105.

[4] The excavation of the floor is not finished.



IRON AGE (A) HUT CIRCLE AT BODRIFTY, CORNWALL

3 feet in from the wall. The posts were in several cases packed with stone and four of the holes produced sherds of the Bronze or Iron Age. There is no evidence of a central post or of a fireplace. Two good stake holes suggest that one part of the outer ring was cut off by partitions running between the posts and the wall. One of these stake holes is overlaid by a later hearth with some paving alongside. This was associated with traces of iron working and is of later date.

The finds include a saddle quern, two mullers, various rubbers and whetstones, flints, beach pebbles and several spindle whorls of a very gritty pottery. These were found at a low level and resemble some from Plumpton Plain.[5] Over 300 sherds were found at various levels, including fragments of the Late Bronze Age (Deverel-Rimbury type) and Iron Age A pottery like that from Dainton, Devon.[6]

Huts of this type, with an inner ring of posts supporting the roof have been found on a number of Iron Age sites including Maiden Castle.[7] In Cornwall they are characteristic of the latest pre-Roman settlement at Castle Dore.[8] The form of the roof is well illustrated by the reconstructed drawing reproduced by Dr Peate.[9] This more primitive Finnish example shews a conical roof of poles supported on horizontal timbers carried by the upright posts. The lower ends of the poles there rest on the ground. At Bodrifty they would have rested on the inner edge of the hut wall as in the black houses of the Hebrides.[10]

D. DUDLEY
West Cornwall Field Club.

THE ORIGIN OF MAIZE

In ANTIQUITY No. 99, p. 157, I made some comments on Mr Carter's article on this subject. In it he mentions a paper by Stonor and Anderson which he regards as striking at the root of the problem. I now wish to call attention to a reply to this paper, which I believe to be a complete refutation of it, by P. C. Mangelsdorf and D. L. Oliver, (*Botanical Museum Leaflets*, Harvard University, vol. 14, no. 10, 1951, pp. 263–91) entitled 'Whence came maize to Asia ?' Mangelsdorf is well-known as one of the foremost authorities on the origin and genetics of maize, and Oliver is Associate Professor of Anthropology at Harvard.

On the botanical side, the main points on which they differ from Stonor and Anderson are as follows :—

(1) The maize found in the Assamese Hills is not unique among living varieties and comparable only with American fossil varieties. It is still found in America and elsewhere.

(2) There is no record of maize in Asia before 1492.

(3) The distribution of the varieties of maize in Asia does not imply that it was there before 1492. It can be explained better on the hypothesis of a more recent introduction.

(4) The resemblances between sorghum and maize from Assam and from Bat Cave are superficial and of no significance in the argument.

On the ethnological side they show that the evidence quoted by Stonor and Anderson to prove the antiquity of maize in Assam has been misinterpreted and does nothing of the kind.

G.H.S.B.

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[5] *Proc. Prehistoric Soc.*, 1935, 32–3.

[6] The pottery is still under examination.

[7] R. E. M. Wheeler, *Maiden Castle*, p. 95, fig. 18.

[8] *Journ. Royal Inst. Cornwall*, N.S. i, Appendix.

[9] I. C. Peate, *The Welsh House* (Y Cymmrodor, vol. XLVII), p. 49.

[10] ANTIQUITY, XII, 264.

THE 'DARK AGE' IN NORTH WEST INDIA

The two outstanding problems of prehistoric N.W. India are those of finding convincing links between the Indus and the Middle East, and of finding any evidence at all for the period between the end of the Indus Civilization[1] and the beginnings of sound history in India about 500 B.C.[2] Faced with these problems, archaeologists have tended to exhibit an academic *horror vacui* far more devastating than its natural counterpart. The most unlikely materials have been cast in to plug the breach. The prehistorian, for example, appears content with a handful of Harappan seals from Irak as evidence of trade and contact between the Sargonid realm and the unknown rulers of the Indus. Here—as in other archaeological fields—a background of work on a historically attested period seems necessary to maintain a balanced judgment when handling purely 'archaeological' material. The visitor from the court of the Caliph Harun-al-Rashid would have felt himself as much at home in the ports of Sind as does the modern European in Karachi. The envoy of Naram Sin or Hammurabi, by contrast, would have found the Indus Valley a strange new world, in some ways more advanced than his own, but in general more backward.

The latest attempt to fill this double breach is that which claims to find provincial variants of the culture of Sialk Cemetery B in the newly defined 'Londo' ware and the long-known Moghal Ghundi cairns of Baluchistan and which assigns a date about the beginning of the first millennium to these cultures. The theory has been advanced by Colonel Gordon and Miss de Cardi, and has received the seal of canonical approval from Professor Piggott.[3] As a stop-gap it is undeniably attractive ; but in one case it certainly will not hold, and in the other the cracks are menacing.

The Moghal Ghundi cairns were discovered by Sir Aurel Stein, who excavated a number of them.[4] Some appeared to be cenotaphs ; others yielded cremated bones and some grave furniture. The pottery was of brittle fabric, light pink inside, white slipped externally, and unpainted ; with one significant exception, to which we will return, it has no determinate parallels outside Baluchistan. The metal objects found included three-leafed arrow-heads, and spear- and knife-points in iron ; 'catbells',[5] a tripod jar, and rings of bronze ; and an expandable silver bracelet. In default of other evidence, the bells and the tripod jar have been related to those of Sialk B, and parallels for the three-leafed arrow-head have been sought in Anatolia. But it seems rash, to say the least of it, to seek parallels so far afield when they can be found nearer at hand. Scarcely two hundred and fifty miles from Moghal Ghundi, the Scytho-Parthian and Kushan city of Sirkap, Taxila, has produced arrow-heads and tripod jars[6] which resemble those from the cairns at least as closely as do the similar objects from Sialk.

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[1] Dated nowadays to *ca.* 1500 B.C., e.g., by Piggott, *Prehistoric India,* 144 : but this date may be as much as 500 years too low. (See, however, ANTIQUITY, XXIII, 201–5. ED.).

[2] The detailed exposition by Pusalker of the dynastic traditions contained in the Puranas (Majumdar and Pusalker, ed., *The Vedic Age,* Book IV) serves only to show how valueless these traditions are for the historian.

[3] For a general account, Piggott, *Prehistoric India,* 240 ; for Moghal-Ghundi, Gordon, *Man in India,* XXVII (1947), 234 ; for Londo ware, de Cardi, *Iraq,* vol. XIII, part 2 (1951), 63–75.

[4] Stein, *An Archaeological Tour in Waziristan and Northern Baluchistan,* Memoirs Arch. Survey India, No. 37, 43–9.

[5] The 'cat-bells' were not found during excavations and are only considered 'likely' to have come from the cairns ; Stein, *op. cit.,* 47.

And there is other evidence which must be conclusive for the later date. Among Stein's finds at Moghal Ghundi he particularizes a bronze ring with an engraving of a helmeted male figure carrying a bow and spear, accompanied by a dancing woman.[7] About this ring there is a conspiracy of silence among our modern commentators; they come forward with no parallels from Nihavend ceramics or Assyrian glyptic. But this same silence may be interpreted as a tacit admission that Stein—who knew about these things—was right in assigning the ring to the Kushan or Gupta periods. And a similar date, or one only slightly earlier, may be ascribed to a pot decorated in relief with bunches of grapes, palmettes and figures holding festoons.[8] These two details together with the general similarity of the metal objects from Sirkap and Moghal Ghundi, provide convincing evidence for dating the cairns, in round terms, to the period 200 B.C.–A.D. 200.

The second of our stop-gaps, Londo ware, is unfortunately known only from surface collections made by Stein and the equally intrepid Miss de Cardi. It is doubly unfortunate that only a small selection of pot forms and motifs has so far been published.[9] But from a study of the Londo ware in the possession of the Archaeological Survey of Pakistan, it can be said at once that there is no resemblance between the fabric of Londo ware, which is very heavily grog-backed, and that of Sialk B, which is described as being fine[10]; that the distinctive forms of Sialk B, especially the 'teapot' do not occur in Baluchistan; and that the leading Londo motif, the pot-hook spiral, is not known at Sialk. Other comparisons between the Londo metopic designs and those of Nihavend are too generalized to bear much weight. There remains one positive link between Londo and Sialk: a stylistic comparison between the horses on sherds from Londo and the animals on the Sialk 'teapots'. Now the subjective judgment of style would carry more conviction here had the objective identification of these animals been made correctly in the first place. Miss de Cardi calls them horses; but extensive zoological research coupled with a Gallup poll among men of peasant stock suggests that horses are not, in the course of nature, provided with horns.

It may be added that the distribution of Londo ware also argues against its derivation from Sialk. The danger here lies in regarding archaeological finds as so many 'type-fossils' without reference to the activities of the men who used them and spread them. Stein's reconnaissances in S.E. Iran and Sistan[11] have shown that the Sialk cemetery B people did not migrate slowly from South West Persia to the eastern Makran, for such a migration would unfailingly have left its mark. Of course a 'horsey' people such as they were could, under pressure, have migrated with the greatest speed, leaving no trace of their passage. We might then have expected their culture to be carried undiluted from the old ground to the new ; but the relation between the Londo and Sialk ceramics is most emphatically not that between, for instance, British and Continental Beakers. The remaining possibility, that these horse-breeders and cattle-rearers took ship down the Persian Gulf and sailed to the Makran is too incredible to notice.

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[6] Three-leafed arrow-heads at Sirkap; *Archaeological Survey of India, Annual Report*, 1914-15, pl. X; tripod jars from Sirkap, *ASIR*, 1926–7, pl. XXVIII, 1, and other unpublished specimens in Taxila Museum. Three-leafed arrow-heads have also been found at the Arab port of Bhambor, Sind, where they probably date to the 9th century A.D.

[7] Stein, *op. cit.*, 48. Unfortunately Stein does not illustrate the design of the ring; and the Archaeological Survey of India appears to have no knowledge of it.

[8] Stein, *op. cit.*, 47–8 and pl. X.

[9] In *Iraq*, 1951, with references to painted sherds previously illustrated by Stein.

[10] The Sialk comparisons have been made with the material illustrated by Schaeffer in *Stratigraphie Comparée . . . de l'Asie occidentale.*

[11] Stein, *Archaeological Reconnaissances in N.W. India and S.E. Iran, passim.*; *Innermost Asia*, chaps. XXIX–XXX.

In our present blissless ignorance, one suggestion may be made about Londo ware, but with diffidence. Though grog-backing was used for mixing basins and oven-pots in the Quetta and Saadat cultures, it does not appear to have been generally used for prehistoric wares. Here, as Miss de Cardi correctly observes, Londo ware is quite unlike any known prehistoric ceramic from Baluchistan. But a grog-backing does seem to have been general in a later pottery industry found around Quetta, and dated by Professor Piggott to the Sassan period.[12] Though the percentage of grog is lower and the shape of the particles differs from those in Londo ware, this Sassanian pottery may provide a clue for its dating.

In conclusion, we may suggest that stop-gaps, when too convenient or too readily acceptable, operate as obstacles to the acquisition of knowledge, which can only come by further and intensive attacks on the problems of the Indian dark age. With this in mind, we plead that the case for the derivation of the Londo and Moghal Ghundi cultures from that of Cemetery B at Sialk be judged ' non-proven '. A. ALCOCK.

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[12] Piggott in *Ancient India*, No. 5. Since this article was published further reconnaissances around Quetta have confirmed the attribution of the Lahore Museum pot to the Quetta region, and have yielded further samples of this fabric.

Reviews

PREHISTORIC INDIA. *By* STUART PIGGOTT. *Pelican Books*, 1950, *pp.* 1–293, *plates* I–VIII, *text figs.* 1–32. *Price*, 2*s* 6*d*.

Prehistoric India to 1000 B.C., to use the full title, is the scholastic harvest won from opportunities presented during some four years' military service in India. The achievement of this important book is a measure not only of Professor Piggott's stature as a prehistorian, but is a tribute to the excellence and applicability of British archaeological method as taught and practised at home. In the preface, the author rightly stresses that the book '. . . does not pretend to be more than a stocktaking of our incomplete evidence . . . ', but much original thought, practical work on the pottery, and field investigation give substance to the argument as it develops.

The main body of the work is concerned with the peasant and urban cultures of prehistoric north-western India and her western borders, the fate of these cultures and the advent of the Aryans. The first two chapters, however, provide background studies of the development of Indian prehistory, and of the earliest human inhabitation of the sub-continent. Readers who are not specialists in the Old Stone Age will be grateful for a very helpful summary of the palaeolithic in India, and the possibilities connected with the appearance of microlithic industries in what, in India also, may be called 'post-glacial times'. The third chapter is a splendid essay on the beginnings of food-producing economy, and the cultures expressive of this change, in Western Asia. A description of the sequences and inter-relationships of neolithic and early metal-using cultures of Mesopotamia and Iran follows, and this would deserve careful study for its own sake even if it were not included as an essential prelude to the comprehension of the Indian material.

In dealing with the pottery from ancient village sites in Baluchistan and North-western India, Professor Piggott has found it possible to follow D. MacCowan's classification of the Iranian painted ceramics, and he has distinguished a Buff-ware province, probably including three ceramic groups, or cultures, located mainly in south Baluchistan and Sind, and a Red-ware province in north Baluchistan. There is of course considerable interpenetration of these provinces. The more westerly connections of these cultures are discussed as is the evidence for trade between Early Dynastic Sumer and the Kulli culture of the Makran. The variety of burial methods amongst these painted pottery cultures is noteworthy, and fractional burial is represented.

The long and very full chapter on the Indus valley cities was fortunately not written too soon to miss taking account of Professor Mortimer Wheeler's report on his excavations at Harappā. The identification of citadels at Harappā and Mohenjo-daro, and the suspicion of town walls, now bring these cities into line with the general development of urban organisation in the Ancient East. The mature and static material culture observed throughout the successive phases of occupation in the two cities is peculiar, and indeed depressing, but it strongly suggests that they must have been intentionally founded as colonies of a parent city whose particular culture had already developed. In view of the apparent Red-ware affiliation of the Harappā pottery, the most likely location for this parent city is north-westwards, perhaps near Peshawar. A fascinating problem for future Indian archaeology is to explore the eastward influence of the Indus civilisation.

Surely the principal of zoned cultures, so well demonstrated around other city civilisations, took effect in northern India as well.

The final two chapters are of particular value for they bring together, and co-ordinate, information that has been even more inaccessible than that dealt with in the earlier chapters. Moreover their bearing is in many ways more direct on the interests of Old World prehistorians as a whole for they deal with the collapse of the great cities, the probable identity of the sackers, and the archaeological interpretation of the *Rigveda*. It would be tempting to elaborate a commentary on these matters, but it must suffice to say that the evidence embodied in Rana Gundai IV, Shahi-tump, the Jhukar culture, and Cemetery H at Harappā, points not to far-travelled Aryans but to barbarous peoples from the periphery of the Indus civilisation, who may have had a sprinkling of Aryan leaders (viz the shaft-hole axes), but who had become invaders on account of pressure in their own rear. The situation is similar to that so well known in Northwestern Europe where the ancient invaders of Britain were seldom the initiators of the movement but rather the outer waves of a disturbance far to the south. It seems to me that the close-knit socio-religious communities who composed the Aryans of the *Rigveda* have not yet been discovered archaeologically. The literary evidence for their exclusiveness, and their West Asiatic technological connections (chariot-wheels with spokes and metal tyres, cauldrons, etc.) to say nothing of their position in comparative philology, make it clear that they were a later and genuinely foreign intrusion to the Punjab. Material from Moghul-gundai and Jiwanri, related to the horsemen's graves at Sialk B, does however point the way.

The book is admirably illustrated and the text figures include three maps. A special word of praise is due to the publishers for their promotion of knowledge in securing the writing of this book for their archaeological series. T. G. E. POWELL.

'FORT' AT SCOTSTARVIT COVERT, FIFE, AND RECTANGULAR ENCLOSURE ON GREEN CRAIG, FIFE. *By* GERHARD BERSU. Reprinted from *Proceedings of the Society of Antiquaries of Scotland*, vol. LXXXII for 1947–8 (1950), *pp.* 241–75, *fig.* 13.

Since Dr Bersu's excavations in Wiltshire, at Little Woodbury before the War, gave a fresh start and great impetus to the study of prehistoric houses in the British Isles, he has himself carried out a notable series of excavations in the Isle of Man, Ireland, Wales and Scotland. Two of the most striking facts that he has demonstrated are the extensive use of timber for house building in the Iron Age, and the large size of many of the structures. Various workers since the war have shown that the first is true even in an area so well provided with stone as southern Scotland, but the most elaborate house plan there has been recovered by Dr Bersu's own skill, from tenuous indications in the sandy subsoil at Scotstarvit in Fife. He emphasises the importance of having started the excavation when the soil was wet and slight differences in colour showed up well: one might add that in subsoils more typical of Scotland even scantier traces have probably survived, so that few parallels may ever be found there. Scarcely less striking, though less complicated, than his similar recovery of Viking houses at Ramsey in the Isle of Man[1], this excavation has led to a report which everyone interested in prehistoric houses should read.

The house stood inside a small circular compound, and was three times rebuilt on the same general plan, all apparently in pre-Roman times. It had four elements: an inner ring of eight posts, latterly at any rate enclosing a slightly sunk area; an intermediate ring of posts linked by a timber wall; an outer timber wall, as much as 60 feet in diameter compared with 45 feet at Little Woodbury; and an 'entrance hall', in the first phase 10 feet wide with double doors on the intermediate ring, leading up to the central area.

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[1] *Ant. Journal*, XXIX, 1949.

A valuable feature of the paper is the detailed consideration of how this house is to be reconstructed. Dr Bersu argues, among other things, for a floor at three successive levels, the outermost two feet above ground level—thus linking the site with his larger and later ones at Lissue, Co. Antrim and Ballacagen, Isle of Man, on which the evidence has not yet been published in full. He makes a comparative study of circular house types and, in addition, of the farming economy which may be represented at Scotstarvit.

These discussions are continued in the paper on the post-Roman Iron Age enclosure at Green Craig. Fissured rock and glacial gravel were unfavourable to excavation, but a circular house could be recognised with an outer wall of stone and timber posts, 30 feet across inside, and part of an inner ring of posts.

These reports bring out strongly the importance of *interpretation* at two separate stages in a thorough excavation, first to recognise and group the actual evidence surviving in the soil, and second to give meaning to it. R. B. K. STEVENSON.

EL PROBLEMA DEL CHATELPERRONIENSE (AURINACIENSE INFERIOR) EN ESPANA. *By* F. JORDÁ CERDÁ. (*Cronica del VI Congreso Arqueologico del Sudeste*). *Alcoy*, 1950. *pp.* 63–7 (*Pub. in Cartagena*, 1951).

In a recent paper Francisco Jordá Cerdá has discussed the problem of the Châtelperronian culture in Spain on the basis of his detailed studies of the collections from seven localities where the presence of this stage has been cited in the literature. Approaching the question from point of view of the concept of the survival of industrial techniques, and the blending of these with more advanced influences, he concludes that the Châtelperronian, or Lower Aurignacian of the early workers, does not exist either in the Levant or in the rest of Spain.

On the basis of Breuil's classificatory scheme for the Upper Palaeolithic[1], the following Spanish localities, sporadically distributed throughout the peninsula, have been considered as yielding evidence for a Châtelperronian occupation: the Abric Romani and Estación Agut in Catalonia, the Cueva de Benidoleig (or 'les Calaveres') in Alicante, the open-air sites of Loma del Rubio, Venta de las Navas and Llano de la Venta de las Navas in Granada, and the Caverna de Hoyo de la Mina in Malaga.

With regard to the Catalonian sites, Jordá Cerdá does not consider that a true backed blade industry is present in the upper level at the Abric Romani. Rather he feels that the assemblage from the latter horizon can best be explained as a degeneration of the Mousterian (typically represented in the lower level at this site), which has been influenced from Aurignacian (or Middle Aurignacian) sources, on the basis of the occurrence of various forms of scrapers. The nearby locality of Estación Agut, also excavated and published many years ago, has yielded a series of atypical pieces which makes its attribution to a particular stage very difficult. However, Jordá Cerdá suggests that the industry is connected with that of the Abric Romani.

The Cueva de Benidoleig, further to the South in Alicante, has yielded a clearly defined Mousterian level at the base, and an upper stratum with a Solutrean industry. Although it has been claimed that the intervening horizon produced an industry of Châtelperronian type, Jordá Cerdá's impression is that here again one is dealing with an evolved Mousterian, although the quality of the material does not permit him to compare it with the corresponding horizon at the site of Còva Negra, near Játiva[2] in Valencia.

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[1] Abbé H. Breuil, *Les subdivisions du paléolithique supérieur et leur signification* (Congrès Internat. d'Anthr. et d'Arch. Préhist., C. R. de la XIV Session). Genève, 1912. pp. 5–78, 2ème édition, 1937.

The fact that the three open-air localities of Granada are extremely poorly documented makes it difficult to present a critical study of the materials from them. But Jordá Cerdá remarks that there is no indication of the existence of a backed blade industry at any of them. Here again he finds that the evidence favours the view that a surviving Mousterian has been influenced by true Aurignacian, on the basis of the typology of the several series recovered.

The Caverna de Hoyo de la Mina in Malaga presents a somewhat more complicated picture. Although a few points considered by the excavators to be of pseudo-Châtelperronian type are present, Jordá Cerdá states that the industry is basically Gravettian. Also there are such typically Aurignacian forms as keeled scrapers in the same level[3]. But almost nothing is known concerning how long such older elements as these persisted. Furthermore, Jordá Cerdá reminds us that at Hoyo de la Mina we are dealing with an economy based on fishing and the collection of edible molluscs, rather than hunting as at most other sites of this stage, a fact of prime importance in considering the typology of the flint artifacts.

Both in the centrally located Valley of Manzanares and the relatively well investigated Cantabrian region the Châtelperronian is completely lacking. Sites in the former area show that Mousterian assemblages are overlain by Gravettian, pre-Solutrean and Solutrean levels, while in Cantabria, Aurignacian and Gravettian horizons are superimposed on the Mousterian without any intermediate level.

Jordá Cerdá does not rule out the possibility that the existence of the Châtelperronian will ultimately be demonstrated for Spain, but he emphatically states that this cultural manifestation failed to penetrate the region south of the Pyrenees on the basis of the evidence available at present. For here the chronological equivalent of the Châtelperronian of Western and Central Europe seems to be an evolved and somewhat 'degenerate' Mousterian, which persisted and ultimately became mixed with true Aurignacian elements. On this basis the Gravettian represents the earliest influence of the backed blade cultural tradition to be felt in Spain.

The importance of these facts, which indicate a more strictly limited area of distribution of the Châtelperronian, is of great significance. The problem also gives rise to interesting speculations concerning contemporary developments in North Africa, but these questions, especially the possible affinities between the developed Aterian of the Western Morocco—Tangier region on the one hand, and the Solutrean of Valencia on the other[4], are of much too complex a nature to be discussed here.

HALLAM L. MOVIUS, JR.

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[2] Jordá Cerdá, F., 'La Còva-Negra de Bellús (Játiva) y sus Industrias líticas', *Arch. Prehist. Levantina*, vol. II, 1945, pp. 11–29; 'Nuevos Aspectos paletnológicos de Còva Negra (Játiva)', *Serv. Invest. Prehist.*, Serie de Trabajos, Varios, Num. 6, Valencia, 1947, pp. 19–26

[3] In this connection, the present writer has recovered several Châtelperronian points, in addition to two steep scrapers, at La Colombière (Ain) in direct association with an otherwise overwhelmingly Gravettian assemblage.

[4] Caton-Thompson (Miss) G., 'The Aterian: its Place and Significance in the Palaeolithic World', Royal Anth. Inst. Gt. Brit. and Ireland, *Huxley Memorial Lecture for* 1946. 44 pp.

EXCAVATIONS AT NESSANA, Volume II, Literary Papyri. *Edited by* LIONEL CASSON *and* ERNEST L. HETTICH. *Princeton University Press (London: Geoffrey Cumberlege)*, 1950. *pp.* XIV+175, *with* 8 *plates.* $7.50 (*English Price*, 48*s net*).

This volume is the first to appear of three which will contain the report of the excavations at Nessana, the modern 'Auja-el-Hafir, in the Negeb in Palestine, conducted by Harris Dunscombe Colt, Jr. They are being published under the general direction of the Colt Archaeological Institute, of which Mr Colt is President. In a foreword he states that Volume I will be devoted to reports on the actual excavations, architecture, pottery, coins, glass and miscellaneous small objects, while Volume III will publish the non-literary papyri.

Nessana originated, it seems, as a halting place for caravans on the route from Aila, the modern 'Aqabah, on the Red Sea, to Gaza on the Mediterranean. During Roman and Byzantine times a fort here controlled the southern borders of Palestine. The place reached its greatest prosperity in the 6th and 7th centuries, despite the Arab conquest of Palestine in 634. The ruins of the old town in the plain were demolished by the Turks in World War I, but the Byzantine fortress on the hill above suffered less. The fort included a small church and from a room attached to this, used as a convenient place in which to dispose of rubbish, most of the papyri were recovered. The non-literary documents range in date from the beginning of the 6th century to the end of the 7th and the literary hands, according to the editors, fit very well into these limits. The survival of the papyri in these circumstances borders on the miraculous, but it would be foolish to expect that such a place at such a date could contribute anything important to our knowledge of Greek literature in the proper sense. Actually of the thirteen texts published no less than eight are Christian; these include portions of three codex MSS of the New Testament, viz. fragments from various parts of John, John XVI, 29–XIX, 26, and fragments from the Pauline Epistles. More exciting are fragments from a codex containing the Acts of St. George. Enough survives to show that Krumbacher's theories about this famous legend need some modification. Still more important is a sizeable fragment from the well-known correspondence between Abgar, toparch of Edessa, and Christ. This document, ' next to the *Epistula Abgari*, the fullest Greek version of the correspondence ', is particularly interesting because it seems to throw light on the symbols XMΓ common in Christian papyri. Three texts are concerned with pagan literature; they comprise a Latin-Greek glossary of the Aeneid, fragments of Aeneid II–VI, and a Greek glossary. The most interesting is the first. This contains a continuous text for considerable portions of Books I and II, nothing of Book III, but a selective glossary to Book IV, 248–497. It is far more extensive than any of the similar Latin-Greek glossaries of Virgil, but, like the rest, it is a rather pedestrian document, full of errors, misunderstandings, and variants, the result no doubt of many copyings by more or less illiterate scribes and of continuous use since its archetype was first composed. Nevertheless it is interesting both for comparison with the manuscript tradition of Virgil's text and also for the light which it throws on the later development of the Greek language. The editing, as of all the texts, is extremely careful and thorough and much useful knowledge may be gleaned from the commentary. Detailed criticism would be out of place. I merely observe that at 699 the supplement of the Greek for *quin* is unconvincing (perhaps μ̣ε̣νο̣υ̣ν̣γε) and at 804 the scribe wrote or should have written ʾεκβρασθεντα. The next text, fragments from Aeneid II–VI, offers nothing of particular interest nor does the fragmentary Greek glossary, a very miscellaneous and probably amateur selection from a larger lexicon. Two short legal fragments complete the list. The book is admirably produced.

E. A. BARBER.

DER RÖMISCHE SCHATZFUND VON STRAUBING. *By* J. KEIM *and* HANS KLUMBACH, (Münchner Beiträge zur Vor- und Frühgeschichte, Band 3), 4to *pp.* I–VIII, 1–42, *pls.* 1–46, *figs.* 1–2, 1 *map*; *C. H. Beck'sche Verlagsbuchhandlung, Munich,* 1951; *Price, DM.* 18.50.

The very notable hoard here described was found during building operations in October, 1950, just under two miles southwest of the Roman fort of Straubing-*Sorviodurum* on the Danube. It included parts of eight parade helmets, five greaves, eight chamfrons, seven bronze statuettes, four bases for statuettes, a few miscellaneous bronze objects and a very diverse mass of iron implements, including weapons, tools, hippo-sandals, fittings and nails, all found close to or covered by a bronze camp-kettle of Roman military type. The great variety of the objects indicates that they are a collection of scrap-metal, comparable in diversity with such cauldron-hoards as the Santon Downham, Carlingwark, or Blackburn Mill hoards in Britain. That some of the Straubing objects were of exceptional beauty has nothing to do with their appearance in such a hoard. But the context of the hoard is obscure. The find-spot was within 130 yards of a Roman *villa*, but in ground honeycombed with remains of more primitive settlement, and the real relationship of the objects to these adjacent structures is unrevealed.

Four of the visor-masks have something in common with the well-known British examples from Newstead, though their features, which are less stylized, with intense brows and hair blocked out in Pergamene style, owe much to Hellenistic fashion and less to Western art. Only one head-piece, from a helmet otherwise unrepresented, exhibits a coiffure readily comparable with the Newstead examples. Three further visor-masks are much more remarkable, with hair fantastically dressed in a high peaked coiffure with small tight curls and central parting, essentially Oriental in type. Their physiognomy is also Eastern. The noses display a marked hook and the mouths are small, tight and down-turned, with bronze vertical strips between parted lips, creating the illusion of wide-spaced teeth. Remarkable though the visor-masks are, the associated accoutrements are, however, no less notable and even rarer. In the Straubing hoard there are five greaves. Two are said to form a pair, and certainly bear inscriptions of *turma* or *turmae* named after the same two decurions, but the lack of symmetry in minor detail leads this reviewer at least to wonder whether they are not single pieces from two very similar sets, once allotted together to the same successive squadrons. The other three pieces are certainly single examples from lost pairs, and all were regimental equipment, issued to different squadrons or officers at different times, as is shown by the occurrence of no less than four *graffiti* on one piece. On these pieces there is full scope for a fine range of symbolic decoration. Mars, Hercules, Minerva, the Dioscuri and Ganymede, Jupiter's favourite, figure on greaves and knee-caps, all with intimate reference to the Roman army and its divine protectors. Comparable decoration appears also on the chamfrons, which comprise five and a fragment covering the full head and two which protect the eyes and forehead only. All except one of the larger chamfrons have basket-pattern eye-shields, like the actual examples from Chesters and Corbridge or the figured examples assigned to Sarmatian cataphracts on Trajan's Column. The exception has for an eye-piece a pierced Gorgon-head, and one of the smaller chamfrons has similarly a pierced head of Ganymede. The eyeshields of the smaller pieces also have a pointed terminal like those of *peltae* and one is similarly finished with an eagle's head.

The intriguing Eastern visor-masks bear no inscriptions. All the rest of the inscribed equipment, comprising one visor-mask, one head-piece, three greaves, one knee-cap, and four chamfrons, bear the names of *turmae* or their officers. The proportion is so large as to suggest that all came from a mounted regiment. Thus, while at first sight it is tempting to connect the Oriental visor-masks with the *cohors Canathenorum milliaria sagittariorum*, the 2nd-century garrison at Straubing, there is no suggestion that this unit was *equitata* and such visor-masks seem restricted to cavalry. It is perhaps, then, possible that in the parade different teams were marked by different helmets. If this is so, however, it is odd that decisive evidence of the kind has not previously appeared, though there were, indeed, bronze faces and iron faces at Newstead.

But how came such a miscellaneous collection to form a hoard of scrap metal? The authors base upon the associated statuettes of deities, the suggestion that they had been dedicated in a shrine. But this explanation will hardly cover the miscellaneous iron objects, and it seems clear that the hoard was formed from various sources, whether by looting or by purchase as scrap-metal. If they were loot, some of the objects may indeed once have been dedicated in shrines. But if, as seems most likely, they were scrap-metal, this explanation will hardly serve, and it must be assumed that other parts of the helmets (perhaps the head-piece) had been so severely damaged as to put out of use each piece of which, significantly enough, we only have part. This observation is merely one more guess added to those already made. The outstanding value of the hoard lies in the intrinsic interest of the objects which it contains. They form an unsurpassed collection of Roman parade-armour and the authors are to be warmly congratulated upon the promptitude with which they have produced a succinct, well-illustrated and objective description of the entire collection. I. A. RICHMOND.

THE CITY OF AKHENATEN, Part III. The Central City and the Official Quarters. *By* J. D. S. PENDLEBURY. *Published for the Egypt Exploration Society by Geoffrey Cumberlege, Oxford University Press*, 1951. *Volume* I, *Text, pp.* XVIII *and* 261; *Volume* II, *plates*, CXII. *Price*, *£*9 15*s*.

This is the third volume dealing with the Egypt Exploration Society's excavation of the great city of Amenophis IV (' Akhenaten ') at El-Amarna, a site which had been explored for many years before World War I by a German Expedition, and was taken over by the Society in 1921, previous reports being published in 1923 and 1933 respectively. The present memoir is concerned with the central part of the ancient city, which contained most of the important buildings, notably the Great Temple of the Aten, the Official and Private Palaces of the King with the Royal Chapel, and the Records-Office, police and military barracks, and storehouses. Part of this area had already been uncovered by Petrie in 1891–2, and it was here that he found the famous pavement which was to many the first introduction to the characteristic art of the period. The task of the editor has been rendered exceptionally complicated by the death of Mr Pendlebury in 1941, and by the delays due to the War, so that the work of no less than seven seasons with several different field-directors has to be dealt with here; Professor Fairman and his colleagues are to be congratulated on the production of this complete and satisfactory record in very difficult circumstances.

Perhaps the most interesting features of the volume under review are the detailed plans and reconstructions of the various buildings, giving an extremely good picture of the central city as it appeared at the close of the nine years of its existence, dominated by its temples, gardens, and palaces with their gaily-decorated private apartments. Representations of such public buildings occur frequently in the tomb-chapels of the contemporary officials buried in the cliffs behind the town, which have been exhaustively copied by Mr N. de G. Davies in his six volumes of *The Rock Tombs of El-Amarna* (1903–8). It has now been found that these correspond in detail to the various parts of the Temple-complex recently discovered, of which perspective drawings are given, so that it has been possible to identify and interpret the former in a most remarkable way. Here a word of warning may not be out of place. The visitor to El-Amarna today will be sadly disappointed if he expects to find an Egyptian Pompeii laid out for his inspection : only under the personal guidance of someone who has taken part in the excavations can he hope even to recognize the main landmarks of the city, and a far better idea of the site will be obtained by a study of the admirable air-photographs on plates XXIV and XLV.

Royal palaces in Egypt are much rarer than temples, as being built of mud-brick they have mostly disappeared completely : indeed the Great Palace at El-Amarna is the only stone edifice of the kind, and is here stated to be the largest secular building in the ancient world. By a curious coincidence a somewhat similar palace at Thebes built by Amenophis III for Queen Teye, and excavated by the New York Metropolitan Museum over thirty years ago, has just been published in instalments by Mr Hayes (see *Journal of Near Eastern Studies*, 1951), and it is instructive to compare the account of this almost contemporary building in which Akhenaten spent his youth with the constructions of his own reign at El-Amarna. At both sites an enormous number of jar-labels were discovered, many with dates and names of buildings, which are of considerable importance in supplying historical details of the period, and the main chronological results of this study will be found on pp. 152–60 of the present work ; the record of the Amarna Collection at University College, London, on pp. 224–33, will also be welcomed by students to whom this material has hitherto been unavailable. The general production of these volumes is of the high standard which we expect from the Oxford University Press. ROSALIND MOSS.

EXCAVATIONS AT OLYNTHUS. Part XIII. Vases found in 1934 and 1938. *By* DAVID M. ROBINSON. (*The Johns Hopkins University Studies in Archaeology, No.* 38). *pp.* XIX+463, *with* 267 *plates and coloured Frontispiece. Baltimore : The Johns Hopkins Press. London : Geoffrey Cumberlege ; Oxford University Press.* 1950. *Price,* £10 *net.*

This volume, published partly with the aid of a grant from the University of Mississippi, is a supplement to the fifth volume of the record of the Olynthus excavations, in which the vases found in the campaigns of 1928 and 1931 were published. Those here recorded were the yield of the years 1934 and 1938. Hurriedly packed up and buried during the war, they were finally redeemed and repaired, in spite of infinite difficulties, by the labour and devotion, and mainly at the personal expense, of Professor Robinson, who may claim, like a new founder, to have restored Olynthus to modern times in these volumes, which are a veritable ' monumentum aere perennius ' to himself and to the city.

More than eleven hundred vases and fragments are described and annotated and everything of significance is illustrated in the plates at the end of the book, which is produced in the sumptuous style with which we are familiar in these records. A large variety of ware is represented, both in the type of vessel and the manner of its decoration, where it has any. The ceramic finds at Olynthus extend in time from the vases and sherds of the Neolithic settlement (3000–2500 B.C.), discussed in Vol. I, to the painted pottery of 460–348 B.C., the date of the city's extinction, although there is no Minoan pottery, as there are no Bronze Age finds. Much of the pottery belongs to the period 420–343 B.C. and the fixed *terminus ante quem* has helped in dating similar material from other sites. particularly from the Athenian Agora. Vases of notable artistic merit are naturally rare, but there are a few, such as the early red-figured calyx-crater with finely drawn figures of a Nike and a warrior on one side, and a young warrior with two spears pursuing a woman on the other. Unfortunately it is much broken, so that only portions of the figures survive. Professor Robinson claims that it is probably by the painter of the Deepdene amphora, and attributes a number of vases figured in this volume, or, more frequently, in Vol. v, to the painters ' created by Beazley '.

Professor Robinson points out that most of the Olynthian pottery is related to wares which were abundant in Rhodes and cites many parallels. Many of the vases are of local fabric and naturally the great majority of them are undecorated. A surprisingly large proportion of them, over 90 per cent, are miniatures. ' Why hundreds of miniature vases were used, I do not know ', says Professor Robinson, ' unless the Olynthians, like the Japanese, served many small courses and drinks in such small vases '. This does not sound very convincing, but may possibly be the explanation of a rather puzzling fact.

The plain ware by its abundance and variety affords ample material for the study of the development of the shapes and forms of vases in the late 5th and 4th century up to 348 B.C. and Professor Robinson discusses many types : oenochoae, canthari, scyphi, pyxides, lecythi, asci and others. It would be superfluous to praise either the skill and learning of the author or the magnificence of the production. G.F.F.

EXCAVATIONS AT GÖZLÜ KULE, TARSUS ; the Hellenistic and Roman Periods. *Vol.* 1, *Text, pp.* 420 ; *Vol.* 1, *Plates,* 276 *Pll. and* 6 *Plans. Edited by* HETTY GOLDMAN. *Princeton University Press (London, Geoffrey Cumberlege),* 1950. *Price,* £11 15*s.* 0*d.*

In these two sumptuous volumes we have the final official report of the American excavations at Tarsus in so far as the Hellenistic and Roman periods are concerned. The real purpose of the expedition had been to trace the material cultures of the Cilician plain from the Neolithic down to the end of the Hittite age, for which purpose the site chosen was the great prehistoric mound that rises on the outskirts of Tarsus ; that mound formed no part of the famous classical city and was occupied, when at all, only by workshops and humble dwellings, so that no treasures of Hellenistic or of Roman art could be forthcoming ; in such circumstances Miss Goldman has all the more reason for feeling pride that her expedition, seeking primarily prehistoric material, should have made its contribution to the classical archaeology of the Near East.

The buildings, originally poor, and terribly cut about first by Islamic builders and, in recent times, by the defence-works of the French troops, were of interest only as yielding a certain amount of dating evidence in a site where stratification was always difficult and often altogether lacking ; Miss Goldman's extremely careful methods of field work have wrested from what might have seemed hopeless confusion an ordered chronology which, where it applies at all (and she is properly cautious in its application) cannot be gainsaid ; and it is of course this that gives value to her finds in general. The fixed dating of her ' units ' is, naturally, based on the coins ; of these we have a very full discussion by Miss D. H. Cox which brings out some valuable information regarding the local mint. Different sections of the book deal with the lamps, the stamped amphora handles, the pottery, the figurines and the inscriptions—the last being, as one would expect, few in number and with one exception of little importance. The terra-cotta figurines form an interesting series, the more so because they seem to be nearly always of local make, so that they illustrate the output of one of the fairly numerous provincial factories engaged in this popular trade ; on the whole, as Miss Goldman points out, the Tarsus koroplasts were more closely allied in taste and technique with those of Alexandria than with the more sophisticated craftsmen of Myrina and Tanagra. Of the pottery too a great deal is of local manufacture, sometimes imitating foreign imports, Attic or other, but sometimes carrying on a local tradition.

Miss Goldman, speaking of the plain lamps, remarks that the trend towards subtle type analysis is assuming some alarming aspects, a remark with which the reviewer heartily agrees; but the elaborate detail of her own publication is almost equally alarming. The catalogue of pottery (mostly fragments) takes 86 pages—' all pieces which were given inventory numbers at the excavation are included, even though subsequent finds have eclipsed their original importance '—and they are illustrated by 90 plates of which 50 are photographic. The very full general account illustrated by a selection of typical examples and supplemented by a stratification analysis showing types and numbers instead of catalogue references would have done full justice to what is after all second-class material. The lamps and the terra-cottas are of course more individual but not all the fragments are important nor are slight variations always informative; 20 plates with 50 pages of text for the former and 45 plates with 86 pages for the latter are far too generous an allowance; incidentally, the reader would be helped if scales were given on the plates instead of measurements in the catalogue. The serious objection to this luxury of detail is not merely that the student is suffocated by information, a great deal of which has literally no value; it is that it puts the book out of the ordinary student's reach. There is here a contribution to classical knowledge, but it is not one for which the scholar should be asked to pay £11 15*s* 0*d*; the object of us all is the diffusion of knowledge, but these extravagant prices, out of all relation to real values, defeat that object most grievously.

LEONARD WOOLLEY.

THE VEDIC AGE. (Vol. I of History and Culture of the Indian People. Bhāratīya Itihāsa Samiti). *General Editor*: R. C. MAJUMDAR. *London. George Allen and Unwin Ltd.* 1951. *pp.* 1–565. *Pls.* I–VIII, 2 *folders*, 2 *maps in text. Price*, 35*s*.

This volume of some five hundred pages of text is written exclusively by eleven Indian scholars, and it is of great interest on this account no less than for its subject matter. The volume is divided into seven Books dealing with I, Introductory; II, The Prehistoric Age; III, The Aryans in India; IV, Historical Traditions; V, The Age of the Rik-Samhita; VI, The Age of the Later Samhitas; VII, The Age of the Upanishads and Sūtras. Each Book contains from two to six chapters. There are copious notes and bibliographies.

In the Foreword, the intention and general plan of this and the subsequent volumes is discussed by K. M. Munshi, and in the Preface, by the general editor, R. C. Majumdar, some useful points are made about the nature of Indian History and the misleading practice of dividing it into Hindu, Muslim, and British periods. He proposes the substitution of the terms Ancient, Medieval, and Modern, and suggests that the boundary between the first two should be approximately A.D. 1000, a time of internal decadence and the intrusion of foreigners. The same writer deals with other preliminary matters in the first two chapters. Chaper III consists of an excellent historical sketch of archaeological work in India up to 1944. It was written by the late Rao Bahadur K.N. Dikshit, sometime Director-General of the Archaeological Survey of India, and in describing Prinsep's decipherment of the Asokan inscriptions in 1837, he rightly says it was one of the great epigraphical achievements comparable to the elucidation of hieroglyphic or cuneform. The next three chapters provide welcome summaries of the geology, geography, flora and fauna of India as background material for the human scene.

It is in Book II, The Prehistoric Age, that a most unfortunate but doubtless unavoidable matter comes to light. The archaeology is out of date in regard to information and largely in treatment as well. The outstanding achievements obtained during Dr Mortimer Wheeler's tenure of office as Director-General of the Archaeological Survey of India were evidently published after the main writing of the text, although an appendix to the chapter on the Indus valley civilisation quotes references in *Ancient India* No. 3 (1947). It is, however, of value to read the portions on the correspondences between Harappā civilisation and cult and early Hinduism. Dr Sankalia's account of the Indian palaeolithic is more satisfactory than the later part of his chapter which comprises sections on 'neolithic' and 'copper' ages and, in general, readers will have to turn to Professor Piggott's book (reviewed above) for all archaeological matters. The chapter by S. K. Chatterji on Race Movements and Prehistoric Cultures is one, however, of considerable interest and is clearly set forth. It includes a summary and discussion of Indian linguistic history, racial types, and the origin of the Hindu pantheon. Prof. Chatterji makes a good case for the Harappā civilisation having been Dravidian, and he stresses the basic nature of the Austro-Dravidian element in all subsequent Indian civilisation. One matter of special interest is the possibility of racial and linguistic connections with ancient Mesopotamia. The extraordinarily Indian physical appearance of, for instance, the well-known stone figure in the British Museum of an un-named governor of Lagash, has for long struck the present writer, and in philology some further exploration into the possible connections or borrowings of Dravidian seem desirable. Prof. Chatterji quotes the word *ur*, 'towns', from a list of early Dravidian words.

Book III is devoted to the general consideration of the Aryans in India, and three of its four chapters are written by B. K. Ghosh of Calcutta, one of the most lucid and informative contributors to the volume. In Chapter X he gives a very full historical sketch of the growth of Indo-European comparative philological studies, and the conflicting views that have held the field from time to time. It is interesting to be reminded that it was a 16th century Florentine merchant at Goa who first claimed that a relationship existed between Sanskrit and certain European languages. Ghosh considers only the philological links in the Indo-European problem, and does not appear to know of the work of Vendryes on parallels in religious terminology, and the sociological and mythological correspondences that have been studied by Dumézil and Dillon, amongst others. He seems to think that it is generally accepted that the Mycenaeans did not speak Greek, but the opposite view is in fact held by the best authorities.

Chapter XI is a short but concentrated discussion of the relationship between the Iranians and Vedic Aryans, and many stimulating, if not wholly conclusive, views are offered. The twelfth chapter, also by Ghosh, is an admirable summary of the nature, arrangement, and relative chronology of the Vedic literature. It would have been of great additional interest if some account had been given of the way in which oral tradition was handed down, and what use, if any, was made of written texts until recent times.

A. D. Pusalker contributes Chapter XIII and reviews the geographical and tribal names in the Rigveda and possible modern identifications. The same writer is responsible for the two chapters that comprise Book IV. The Historical Traditions are mainly derived from a body of lore known as the Purānas which are admitted to be, in their present form, some two thousand years later than the earliest events they purport to record. The texts of the Purānas are very corrupt, but are claimed to be not wholly unreliable, and many pages are devoted to setting out a traditional scheme of dynasties and their alleged chronology. The earliest dates are placed in the late fourth millennium B.C., and continue down to include the Bharata War at *c.* 1400 B.C. The whole subject is set forth as if the chronology was meant to be taken seriously, but it is not clear if Dr Pusalker was merely at pains to elaborate a curiosity, and surely, if the Aryan or non-Aryan dynastic histories meant anything for the third and second millennia B.C., some correlation with the Harappā civilisation would emerge. However, the subject is presented as in a water-tight compartment and in its present form invites no confidence.

The last three Books in the volume deal in some detail with the language and form of the various components of Vedic literature, and there are chapters on political and legal institutions, religion, and social conditions. The contributors are B. K. Ghosh, V. M. Apte, and M. A. Mehendale. Ghosh considers, as others do, that inhumation burial was practised as well as cremation in Rigvedic times, but the passage he quotes about the removal of the bow from the dead man's hand would suggest that this was because he had in fact to be burnt, otherwise he would surely have brought it with him into the grave. The chapters by Apte on social and economic conditions are all too brief for their extraordinary interest and value to archaeological studies far beyond India, no less than within it. One would like to know more about the ' kettles ' used for cooking. These presumably are bronze cauldrons, and are an important indication of the techno-cultural horizon of the Aryans. Other matters of special interest are the strongholds (*pur*), and the carts, no less than the chariots. The special characteristics of the *Sūta*, 'charioteer', who drove the vehicle for the warlord finds parallels in Greece and Ireland.

The Vedic Age will undoubtedly do much to increase interest in early Indian studies; it demonstrates, too, the enormous opportunities for further research and competent exposition by Indian scholars, to whose increasing activities students everywhere will wish well.
T. G. E. Powell.

THE ROMAN STAGE. A Short history of Latin drama in the time of the Republic. *By* W. Beare, M.A., Professor of Latin in the University of Bristol. *pp.* xi, 292, *with* 8 *halftone plates and* 8 *text illustrations. London. Methuen and Co.* 1950. *Price,* 25*s net.*

Professor Beare, whose name has long been associated with Plautine studies, presents in this volume a study of the development of all types of Latin drama in Republican times from tragedy to popular farce, considered in relation to the theatre for which it was intended. The difficulty in writing on this theme is that, apart from the plays of Plautus and Terence, the material that remains for us is meagre in the extreme. For example the known writers of fabula togata, 'comedy in native dress', are Titinius, Afranius and Atta. Of Titinius there survive 15 titles and 180 lines, of Afranius there are 44 titles and some 400 lines, of Atta 11 titles and 20 lines.

Professor Beare does his best with these slender resources and conveys a faint flavour of past drolleries in the renderings of some of the titles : Lucubratio, ' Up by lamplight ', Tiro Proficiscens, ' Off for his first campaign', Matertera, 'Auntie', Icta, ' She who gets slapped ', but has to admit that ' our main evidence, that of the titles and fragments, does not enable us to distinguish clearly between the work of these different writers of togatae, or to reconstruct with probability as much as a single plot'.

With tragedy we are in even worse case, for in this field there is nothing to correspond to the works of Plautus and Terence. The plays of Seneca are of doubtful relevance, for they are not only of a later age but are clearly intended for reading and declamation, rather than for stage production. From the earlier period, when tragedy actually occupied the stage and was a live force, we have nothing but titles and fragments. Of Ennius there are 20 titles and 400 lines, of Pacuvius about the same number of lines with twelve titles, and Professor Beare has to make the sad admission that ' in the whole range of Latin drama we have no knowledge of the course of as much as one plot invented by a Latin writer '. Yet in spite of these limitations the study of Latin drama retains a lasting interest, and the plays of Plautus and Terence gain an enhanced importance as the only complete representatives of the New Comedy that flourished in the Greek world of the 3rd century B.C., and the main links in the literary tradition of dramatic entertainment. The discovery in modern times of considerable fragments of the plays of Menander, although it has given us a firmer basis for judgment, does not affect the position of the Latin writers of Comedy as the main vehicle for this literary form for later ages. Supposed rules of composition, drawn up by late Latin grammarians like Donatus, had immense influence on European drama, but Professor Beare shows, from the evidence of the extant plays and from what we can glean of stage conditions, upon what a slender basis they often rest. In an interesting chapter he discusses the law of five acts, which can claim the authority of the Ars Poetica, and concludes that ' the five act theory seems to be nothing but the product of Roman pedantry, based ultimately on Horace's practical hint—possibly derived from some remark of Varro '.

The book is packed with information, but very readable, and is an admirable compendium of all matters connected with the Roman stage—the dramatists and the extant remains of their work, the theatre, stage buildings and conditions of production. Illustrations of vases and architectural remains are used in evidence and the whole treatment is critical and scholarly. In many of these matters the absence of exact knowledge has given rise to a large literature on controversial questions which Professor Beare handles with a skill that avoids dullness. Some of the more technical subjects, such as the change of scene and scenery, the ' angiportum ', the doors on the stage and the curtain, are treated in appendices, consisting of papers by the author reprinted from Classical periodicals. The book can be strongly recommended to all students of Roman life and letters. G.F.F.

EXCAVATIONS AT ISLAND MACHUGH. *By* O. DAVIES. Supplement to *Proceedings of the Belfast Natural History and Philosophical Society*, 1950, *pp*. 124, *figs*. 47, *pl*. 8. *Price*, 15*s*. *Northern Whig, Belfast.*

What Dr Davies describes as the last major report on his archaeological work in Ireland, deals with the important sequence of stratified deposits uncovered by careful excavation on an island about 180 by 120 feet across, situated just west of the centre of Ulster.

A specialised and probably late form of Neolithic B pottery was freely used by the first inhabitants of the natural tree-covered island. Long cord impressions are a feature of the decoration. The commonest flint tool was the hollow scraper. Axes were scarce, but one wonders what evidence there is for the suggestion that most stone axes in the British Isles were hoes. The small diameter of the numerous remains of posts, associated with cobbling, may indicate flimsy structures. No plan, however, was recognisable. Dr Mitchell's pollen analysis places this occupation at the end of the Sub-boreal climatic phase (Sub-zone VIIb), indicating that it is no earlier than some finds of Early and Middle Bronze Age date from the south of Ireland.

After an interval when trees grew undisturbed, users of Late Bronze Age pottery, including a cordoned urn, constructed a palisade wall round the island, and laid sand and cobbles on a foundation of brushwood over most of the interior with planking and brushwood alone at the edges. Scattered over the area were found stumps of posts, and near the centre a large hearth. But no picture emerges of the roofed structure(s) that may be presumed, particularly from two squared beams continuously slotted for upright boards, and others with mortise holes. The excavator suggests that the greater part of the area was roofed except where a platform had been formed by tumbling logs into the water at one corner of the island. A bronze socketed axe and socketed sickle should belong to this phase.

Palisading, posts, plank and brushwood floor, were repeated at some later date, to be estimated from the conflicting finds: parts of two 'souterrain' pots dated elsewhere to A.D. 500–1000, together with an equally late type of wooden whorl and iron nails, have to be balanced against fragments of a late 'cinerary urn', with typical cord decoration but no cordons, and other sherds of bronze age fabric. Dr Davies estimates a date contemporary with the middle or later Roman Empire, partly on the strength of a distant analogy with pottery of bronze age tradition at Traprain Law. All this is considered under the heading 'Bronze Age Finds', leaving Early Christian for the next stratum, ill-defined in forest peat. For the outsider it is always hard to believe that Irish culture and chronology followed rules of their own, though the view is widely held in Ireland.

After a further interval hearths, iron slag, small moulds and crucibles indicate medieval metal workers who used quantities of shouldered jars with decoration usually confined to the flattened rim. This pottery is widely distributed in Ireland and known as 'crannog ware'. But at Island MacHugh its stratification has been observed for the first time. Not only did it underlie the rubble and mortar tower known to have been built early in the 15th century, but large amounts were found in the upper levels latterly associated with glazed sherds. Thus it seems to have continued for centuries with little recognisable change, at least down to the end of the 16th century. In 1603 the castle on Island MacHugh was 'raised to the ground'—in fact, as often, only slighted—and traces of 17th century occupation were less frequent than of squatters of about 1750–1820.

In an interesting discussion on the suspension of prehistoric round-bottomed pots, Dr Davies considers that they were too fragile to place within a foot of burning wood and contrasts the rims and shoulders of Neolithic A pottery, designed, he thinks, to hold a strap, with the necks of Neolithic B, grooved for a thong or cord.

Such discussions are few, however, and the reader must be prepared for a recital of bare finds and facts whose interpretation is hardly indicated, as in the case of the structural remains or two pages of data on phosphate content. This is no doubt partly due to the pioneer nature of the work, and even more to the unfortunate circumstance, stated in the preface, that the report had to be written abroad without access to the finds, and from field notes and drawings hurriedly executed. Yet the many drawings of pottery could have been greatly improved, if made simply as diagrams without shading or indications of texture, and might have been grouped in the order of the text. It is more regrettable still that the plans are so printed as to make many of the distinctions of period indecipherable. Most purchasers would surely have readily paid a higher price to ensure adequate paper and reproduction, and to be spared having to have the volume sewn before use.

R. B. K. STEVENSON.

EXPLORATIONS IN EASTERN PALESTINE, IV: part 1, text; part 2, pottery, notes and plates. *By* NELSON GLUECK. *Published by the American Schools of Oriental Research, Annual,* vols. XXV–XXVIII (for 1945–9). Publisher's address—Drawer 93a, Yale Station, New Haven, Conn. *Price* 12 *dollars* (2 vols, *postage* 28 *cents*).

This last instalment of Nelson Glueck's *Explorations in Eastern Palestine* gives the results of his inspection of the area north of the River Zerga and in the Jordan Valley.

The book is primarily an inventory of sites. But it is interspersed with the author's reflections on many questions of history, topography and archaeological method. It can thus be described, aptly enough, as a mine of factual material and of earnestly pondered conclusions, both, as is the way with mines, rather troublesome to extract.

Dr Glueck's main theme concerns the fluctuations of urban settlement, and their dependence on human rather than climatic or geographical factors, which he believes to have been constant. Given conditions of peace, intensive settlement based on cultivation has always been possible, both in the highlands and in the Jordan Valley, and has been achieved in certain 'peak' periods of which surface remains are an index.

Arising out of this general theme, space permits only a few points to be noted.

The dolmens, so abundant in northern Gilead, denote a population of 'skilled artisans' which 'spilled over into marginal areas' and developed 'an advanced agricultural civilization' in the 6th millennium 'at the latest'. It would have been interesting to know Dr Glueck's own reasons for now dating the dolmens so early, especially in view of the coincidence noted on p. 75 between their distribution and that of the Chalcolithic settlements. To say that the discovery of Early Bronze Age pottery in some of them 'has no bearing whatsoever upon the dating of the dolmens' (p. 387) is hardly correct; at least it gives a terminus ante quem, disposing, for example, of the author's own earlier dating about 2000 B.C.

Of the 'amazing Nabataeans' Dr Glueck concludes that the northern extension of their kingdom was mainly commercial and that while in the south they irrigated lands never cultivated before or since, in the north 'Hauran and Jebel Druze could not attract land-hungry Nabataean farmers in the same manner'.

On historical or geographical probabilities this may be true, but it does not follow, as Glueck suggests, from the mere absence of that most specialized and least transportable of pottery types, the painted ware so common on Nabataean sites in southern Transjordan.

There is no question that Dr Glueck's survey is an indispensable source of material for the earliest history of Transjordan (no pottery is illustrated later than the Iron Age, and very little of that). Any future synthesis will certainly incorporate many of his conclusions. The identifications of biblical sites in the Jordan valley are, also, convincingly demonstrated. It is a pity that reading the book is so like exploring the author's field notes, even down to the abbreviations. (n for 'north', HM for 'hand-made', and so on).

Discussions of general conclusions, valuable in themselves, are scattered through the book as in a bran-pie, the same points recurring in almost identical phrases in widely dispersed contexts. (cf. p. 326, n. 886 with p. 396, n. 1125; or pp. 191–2 with p. 387). The index is little help, for besides lacking all sub-headings it is far from complete. Thus under 'Chalcolithic' the important pages 74–5, and many other pertinent passages, are omitted; and under 'Hellenistic' the paragraph on p. 353.

The author's own summary, a single short page with no references to the foregoing chapters, is inadequate; by absorbing some of the preceding discussions it could usefully have been expanded into a full chapter.

Facts are no easier to get at. The text, with rare exceptions, gives no references to the plates, and the plates none to the text. Between them, the only link, yet a formidable barrier, stands an array of indices, guarded by an impenetrable alphabetical order. This attempts, in defiance of all experience in listing Arabic place names, to give due honour not only to such optional prefixes as '*tell*' or '*khirbet*' (accounting for four-fifths of the names), but also to the definite article with all its assimilations. Consequently a site like Khirbet (or Tell) Sheikh Muhammad can be listed under the K's on pp. 687 and 702, but under the T's on p. 498. All of which makes the book so much harder to use.

The first thing one looks for in a work of this kind is a good map. It is disappointing to find that the pocket at the end of Part II contains only a blank sheet sprinkled with numbers and three place-names. What a pity that room could not have been found (perhaps by scrapping half the photographs of potsherds, made redundant by the excellent and far more informative drawings) for a fully detailed map, in four sheets if necessary, covering the whole area surveyed (which this does not) on a scale large enough for names and contours to be shown, not to speak of rivers, roads and boundaries. Without such a map, for which the air photographs only occasionally compensate, most of the topographical discussion, which forms an essential part of the book, is impossible to follow.

Criticism of the manner of presenting some of the materials here assembled cannot detract from the solid merit of a survey so perseveringly carried out in difficult country. All archaeologists will recognise this book, and the years of pioneer work in the field which it represents, as an outstanding personal achievement. R. W. Hamilton.

L'ÉVOLUTION DE LA CARTOGRAPHIE DE LA RÉGION DU MONT-CENIS ET DE SES ABORDS AUX XVe ET XVIe SIÈCLES. Étude critique des méthodes de travail des grands cartographes du XVIe siècle. *By* M.-A. de Lavis-Trafford, *pp.* 125; 12 *plates. Chambéry: Librairie Dardel* (*London: Sifton Praed*), 1949.

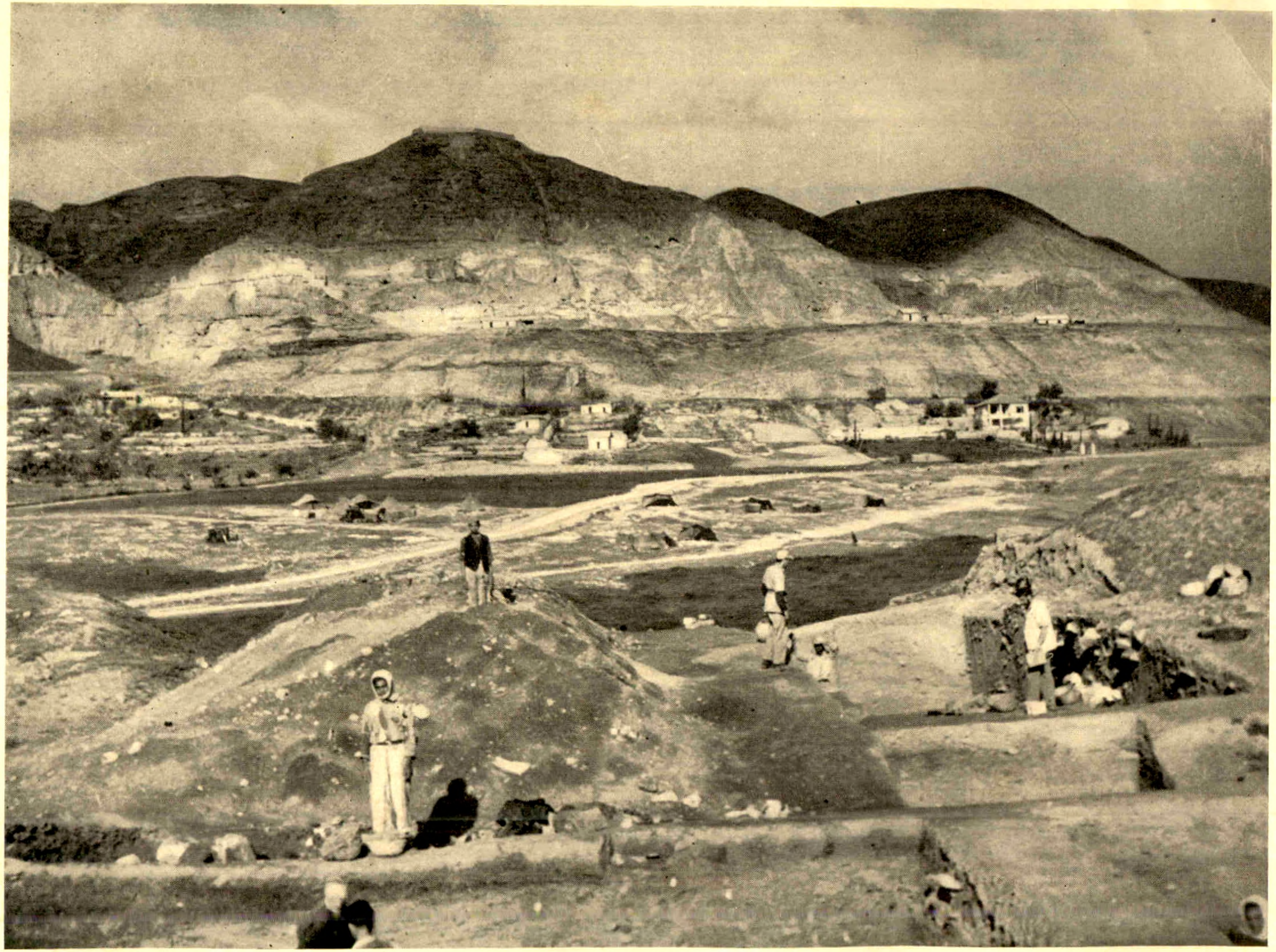
The watershed of the Graian and Cottian Alps, forming the frontier between Dauphiné, Savoy and Piedmont, is a region of violent relief in which the pattern of human settlement and movement is controlled by the upper courses of the rivers—the Isère, the Arc, the Dora Riparia—and by the passages across the mountain barrier. In tracing the ancient routes between 'the two Gauls' which traversed the watershed Dr de Lavis-Trafford has had occasion to search for material in early maps as well as literary and archaeological evidence. This monograph is a by-product of his wider study of the old mountain roads between the Haute Maurienne and the Susa Valley.

To his scrutiny of the representation of the Mont Cenis region in maps of the 15th and 16th centuries Dr de Lavis-Trafford brings an intimate knowledge both of the country and of its archives and literature, manuscript and printed. He rightly supposes that a comparative study of contemporary topographical texts can throw light on the methods of compilation employed by early cartographers. All the maps which he examines, from the printed Ptolemies of the 15th century to Magini, were made by 'géographes de cabinet', or armchair geographers; none is from survey. They are therefore a proper subject for criticism of this kind. Confronted with the topographical knowledge available in their time, the cartographers cut a poor figure. They are convicted, in general, of a superstitious faith in ancient authorities and a frivolous neglect of contemporary sources of information; of an altogether uncritical attitude in weighing and assimilating evidence; and of arbitrary deformation of the hydrography and relief of the region. In the work of individual cartographers, Dr de Lavis-Trafford detects discrepancies between a map and a text by the same author (Signot, 1515) and between maps from the same hand (Gastaldi, 1555 and 1559); inadequate supervision of the engraver (Ortelius); and the exercise of fantasy in filling lacunae due to ignorance of readily accessible information (Mercator). In the period reviewed, Gastaldi's map of Piedmont (1555) is shown to give the most satisfactory version of the Haute Maurienne and of the Mont Cenis plateau: the source of the Arc is correctly identified, and two Monts Cenis are distinguished although misplaced. Gastaldi unexpectedly fails in his delineation of the Italian side of the watershed (the upper valley of the Dora Riparia), for which Orance Finé's map of France (1525) remained the most accurate until Magini's 'Piemonte et Monferrato' of 1608. That the region under examination lies at the extreme limit of political territories (France–Italy; Savoy–Piedmont) partly explains weaknesses in its representation, for an early cartographer's work is often most careless or perfunctory at the edges of his map.

Dr de Lavis-Trafford's detailed analysis of the maps is admirable, and his readers should be grateful for his precise indications of size, method of engraving, present location, and bibliography. His neglect of scale is curious, for this factor governs the selection and delineation of detail in small-scale maps, especially those engraved on wood with their necessarily coarser line and lettering. The scale of no map is given, and the word 'échelle' occurs only twice in his text.

In contrasting the reputations of Gastaldi, Mercator, Ortelius with their patent failures in his field, the author assumes the accents of the little boy who remarked that the Emperor had no clothes on. His argument from the particular to the general, however, rests on too narrow a foundation to command unqualified acceptance. We do not believe that there is a conspiracy to exaggerate the critical sense or scientific discipline of these cartographers. A fair assessment must take account of the familiar fact that the maximum of critical effort is commonly applied at the periphery of knowledge, at the frontier between the known and the unknown. We must mitigate Dr de Lavis-Trafford's uncompromising judgment if we recall (for instance) the care with which the crucial northern passages were drawn and revised on maps, from the most recent reports of explorers.

Nevertheless the author has done a useful service in reasserting certain first principles neglected at their peril by historical geographers and others who use maps as evidence. Maps and documents are complementary, and maps alone are no true index of the state of topographical knowledge at their date. A chronological series of early maps will not necessarily show continuous progress in representation ; its character may be static or even retrogressive. Maps are a conservative, indeed a reactionary, element in the development of geographical ideas : Richard Gough in 1780 estimated that 'the several sorts of . . . [maps] are supposed to amount at least to 16,000 ; but of these not above 1,700 are originals'. Dr de Lavis-Trafford notes the absolute character of statements made by a map in comparison with those of the written word which can more easily be qualified. In ascribing reasons for the survival of errors in maps, he rightly points to the authority lent by the name of a Gastaldi or a Mercator even to their inferior work, but he does not sufficiently emphasise the force of two other factors, the one physical the other economic. These factors are the immensely long life of copper plates and the consequent temptation to pull and sell impressions long after their geographical content had fallen behind contemporary knowledge. A comprehensive study of commercial organisation and relations in the map-publishing industry would throw much light on this and similar problems in the history of cartography. R. A. SKELTON.



JERICHO: LOOKING WEST FROM THE SUMMIT OF THE TELL TOWARDS THE MOUNT OF TEMPTATION

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Antiquity

VOL. XXVI No. 103 SEPTEMBER 1952

Editorial Notes

IF one can imagine a millionaire or a government interested in the history of the human race or of civilization and anxious to promote the study thereof, how could they most profitably invest their money today? The question is not wholly academic; such persons may exist, and one would suppose that these subjects might come within the scope of Unesco. One may assume that the advancement of knowledge is accepted as a desirable end. Once upon a time it was imagined that this end was attained merely by writing books about other books or by speculations about the Infinite and the Nature of Being. That time has passed, though Unesco, which is about to launch a new and expensive philosophical journal, still seems to be living in it. Since modern science began in the 17th century it has become evident, from the results obtained, that knowledge is advanced only by a study of facts. This demands two things—leisure, and apparatus or equipment. Students must be enabled to live, and they must have access to the facts. The former proposition is obvious; research is a whole-time occupation. Access to the facts, in archaeology and many other branches of science, involves field-work (which costs money) and work in museums where the results of field-work are stored. That brings us to the crux.

* *

A museum must be not only large enough to store its contents and exhibit a selection, but the contents must be properly arranged so that they can be studied. One that contains merely packing-cases full of objects is, so long as they remain there, useless. It must have a laboratory for conserving and repairing objects; specimens deteriorate if not treated, and broken pots must be mended. Every museum is at present understaffed; almost the whole time of the overworked staff is spent in mere administration, and much of it is wasted in answering impudent questions about trifling expenditure Only the stoutest can wear down the persistency of ignorant financial clerks and find time for research as well. If a person is fit for the responsible post of Director of Antiquities, he should not have to explain why transport is needed to conduct an excavation. (We have an actual example in mind). Museum staffs, therefore, need enlargement and less pettifogging interference. But when this has been done, there is a crying need for the endowment of students who may not be members of the permanent staff, to study the contents. There are, for instance, hundreds of cuneiform tablets still untranscribed, uncatalogued and often hardly even examined. By ' uncatalogued ' we do not mean unregistered, but the mere block entry of a collection in the museum register of accessions, though quite necessary, is utterly inadequate. Quantities of papyri are in the same condition. The sort of thing that needs doing is what was done by the National Museum of Wales and the London Museum when they published their famous Catalogues.

⁂ ⁂

Something on the lines suggested has been done in a few countries. But it should be remembered that to endow a student for a particular piece of research may impose a strain on an already overstrained staff ; and in any case endowment is premature if the collections to be studied are still packed up : the strain becomes intolerable when, to the duties of a museum curator are added those of a Conservator of National Antiquities. We do not know how far our remarks may apply to the Archaeological Survey of the Union of South Africa, whose Report for 1950–1 has just reached us. Some of the contents illustrate these remarks so well that we quote them here. The Survey is housed rent-free by the University of Witwatersrand, which is to be congratulated for its public spirit in undertaking what should be a government responsibility. But that accommodation is ' totally inadequate ', and cripples the Survey's activities. ' Unless adequate accommodation is provided in the near future, the Survey will . . . degenerate into a purely administrate body. The vast archaeological wealth of the Union, especially that portion of it which is capable of throwing so much light on the earliest chapters of human history, is crying out for systematic exploration. Prehistorians in all parts of the world look to South Africa for intensified exploratory work, and when it is realised that our obligations are of an international rather than of a purely national order, the fact that we cannot meet them because we have neither the accommodation nor the staff we require, is a sad reflection of the ill-balanced times in which we live '.

⁂ ⁂

We endorse every word of this appeal. We do so the more unhesitingly because, as stated, the need is an international one. Prehistory is one and indivisible ; we cannot investigate the problems of human evolution on a merely national basis ; they are as much the concern of Europeans as of South Africans, and Europeans to-day, in the unhappy state of their continent, look more and more to other lands to carry on the struggle. One would not wish to antagonize governments by caustic criticism, however justified ; it is a fact, however, that in two other regions of Africa—Nigeria and Kenya—the governments concerned have failed in their duty to provide accommodation which in both is due to private enterprise of a very laudable nature.

⁂ ⁂

Our first concrete proposal, therefore, adumbrated in the first paragraph of these Notes, is addressed to governments which should start setting their archaeological houses in order. When this has been done students should be assisted in studying certain aspects of the contents. Exactly what form such endowments should take is not for us to suggest except in very general terms ; specialists will be only too ready to fill in the details. But we think there will be a general agreement about the need, and that in voicing it we are merely giving expression to widely held opinions. We can do so freely because we are not inhibited by an official position, and readers of ANTIQUITY who are similarly uninhibited can assist by talking about the subject to persons who can influence the powers that be.

⁂ ⁂

Jericho is in the news again. Just a year ago we reported that it was to be excavated under the direction of Miss K. M. Kenyon, D.LIT., F.S.A., forecasting that 'the excavations may produce results of decisive and far-reaching importance', and for that and other reasons supporting the appeal for funds (ANTIQUITY XXV, 116–7). How well justified the forecast was can now be seen from Miss Kenyon's article in this number. Deeply buried at the bottom of the city mound have been found the walls of houses and the stone wall of a city (PLATE II) belonging to a remote epoch before the invention of pottery. These remains were securely sealed below a thick unbroken neolithic stratum containing potsherds of known neolithic types. These pre-pottery houses are considered to date at least from the 4th millennium B.C. This is a discovery of major importance for the history of civilization; it shews that people built houses before they discovered the art of pot-making. Jericho can now claim to be the oldest known town in the world, with a continuous history of at least 6000 years. It is hoped to carry out two more seasons' work there. We congratulate the Director and her colleagues and supporters, and heartily recommend the excavations to the further support of our readers. Subscriptions to the excavation fund may be sent to the Hon. Treasurer, British School of Archaeology in Jerusalem, 2 Hinde St., Manchester Square, London, W.1.

⁂ ⁂

The more observant of our readers may have noticed a slight change in style on the outside of the cover of this number. This is necessitated by an honour bestowed upon the Editor by the University of Cambridge which on June 5th last conferred upon him the Honorary Degree of Doctor of Letters (Litt.D.). It is a distinction which he is proud to record here, not least because, in his Latin oration, the Public Orator referred to ANTIQUITAS as constituting one of the recipient's claims to recognition. Of the other two mentioned, one was the occasion for a superb pun—*μαψ ἀταρ οὐ κατὰ κόσμον* (*Iliad* II, 214); but Homer was not thinking of archaeology and the Ordnance Survey.

⁂ ⁂

In a moment of temporary aberration I implied—in a bad-tempered and inaccurate footnote (No. 101, March, p. 55)—that the Ministry of Works did not publish accounts of its operations. It does, and I withdraw what I said, with regret for having given offence. The Ancient Monuments Inspectorate of the Ministry of Works (Chief Inspector, Mr B. H. St. J. O'Neil, V-P.S.A.) publishes its results in established archaeological journals. Through the courtesy of the Ministry a list of the chief publications during the last 13 years is printed on pp. 153–4. It is a record of which not only the Ministry but British archaeology as a whole may well feel proud. O.G.S.C.

Early Jericho

by Kathleen M. Kenyon

IN Professor Garstang's two final seasons' work at Jericho, he established the remote antiquity of the origin of the settlement on that site.[1] He showed that beneath the long sequence of Bronze Age levels lay a considerable depth of Neolithic occupation, with beneath it again soil containing flints of the Palestinian Mesolithic period. At Jericho, therefore, there was evidence of the earliest settled occupation hitherto discovered in Palestine, and the underlying Mesolithic levels suggested that the site might provide evidence for the transition from a food-gathering to a food-producing economy.

Early Jericho is thus a key-site for the prehistory of Palestine and Western Asia. Owing to the great depth of deposit, Professor Garstang was only able to penetrate the lowest strata in a restricted sounding, and further examination was anxiously awaited by those concerned with the beginnings of civilisation in the Near East. When plans were being discussed for the resumption of active work in Palestine by the British School of Archaeology in Jerusalem, and its parent body the Palestine Exploration Fund, the claims of Jericho, both on these grounds and those of its association with Biblical history, made it the first choice, and it was decided to apply for permission to undertake a season's excavation there in 1952. The project was welcomed by the Government of the Hashemite Kingdom of the Jordan, and I would like here to express my gratitude for the enthusiastic co-operation of the Government through its Department of Antiquities, and its Director, Mr G. L. Harding. Two members of the staff of the Department, Auni Beg Dajani and Dr Abdul Karim Gharreybeh, participated in the excavations and did most useful work, while the former was responsible for the most sensational discovery of the season. To our great satisfaction, the Trustees of the American Schools of Oriental Research proposed that the School in Jerusalem should collaborate in the excavations. Staff and students of the American School therefore joined those of the British School in the work, and particular thanks must be expressed to Dr William Reed, Director of the School, and to Dr A. D. Tushingham, who acted as Assistant Director of the excavations.[2]

Jericho lies in the Jordan Rift, some eight miles north of the Dead Sea. To the west it is overlooked by the lower cliffs of the Mountains of Judah, with the towering Mount of Temptation almost overhanging it (*frontispiece*), while to the east it looks out over the flat flood plain of the Jordan, with the mountains of Transjordan in the distance. From the west side of the Tell wells out the copious spring which no doubt was the reason for the ancient settlement, while today it waters the fertile oasis of modern Jericho.

This account of the finds of the season's work is necessarily of a preliminary nature. It is clear that further seasons' work are necessary to elucidate the problems fully. Moreover, the material has been only rapidly examined and the full classification must await its arrival in this country. For this reason, no drawings of pottery or flints can be published here.

Professor Garstang reached the early levels in a deep trench at the north end of the

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[1] *Liverpool Annals of Art and Archaeology*, XXII and XXIII.

[2] Generous contributions to the British share of the cost of the expedition were made, in addition to those of the two sponsoring bodies, by the British Academy, Sydney University, Birmingham City Museum, the Ashmolean Museum and the Cambridge Museum of Archaeology and Ethnology, while the Pontifical Biblical Institute also contributed generously to the cost. The members of the staff from the United Kingdom, Australia, Canada and the United States are too numerous to mention by name, but all did most useful work.

Tell, immediately inside the northern wall of the latest Early Bronze Age city (PLAN opp. p. 116, Squares E 6–8, PLATE IIa). The latest surviving level here was Early Bronze III, and the uppermost Neolithic floors (Professor Garstang's level IX) lay at a depth of about 6 m. below the highest point of the Tell here. The main trench was cleared to level XI, about 1 m. lower, and at the west end a pit about 6 m. square at the top, and 2 m. at the base, was carried down a further 6 m. In level IX, the upper portion produced Neolithic pottery, while in the lower part and the preceding levels X–XVII there was no pottery. The 3 m. of deposit below this produced microliths of the Palestinian Mesolithic (Natufian) type.

The characteristic structures of these Neolithic levels were houses with rectilinear rooms and walls of mud-brick, the walls and floors being plastered and burnished. The 1952 expedition continued the clearance below level XI, and the finds are briefly described below. Excavation from the surface of the Tell in a strip immediately to the north was also begun, with the object of uncovering a further area of the Neolithic levels.

In the general examination of the Tell, an area was selected for excavation in the centre of the west side, immediately inside the western line of defences (PLAN opp. p. 116, Squares G 4 and 5, H 4, sites D I and F I). Neolithic occupation was found here again, with pottery identical with that from the earlier excavations, at the surprisingly shallow depth of 1.50 m. from the surface. The latest surviving structures here, in fact, were round houses of mud brick, of the type ascribed by Professor Garstang to E.B.I.

It was thus established that the Neolithic settlement was of considerable size, extending some 120 m. from the north end of the Tell to its centre, and it may of course extend equally far to the south of the centre. On the other hand, a preliminary examination of the pottery did not identify anything comparable with the material published from level VIII of the earlier excavations, called Chalcolithic in the report of the 1935 season, and Late Neolithic in that for 1936. This occupation may therefore have been more restricted in area.

A second fact of importance emerged from the clearance of this area, which was that it seems possible to make a sharper break between the layers with Neolithic pottery and the earlier ones in which flints of Neolithic type are found, but no pottery, than was established in the earlier excavations. The Neolithic pottery here was associated with walls which were largely stone-built in so far as they survived. The building style was rough, of undressed stones. Four phases of structures, all associated with the same general type of pottery, could be identified. It is too early to say whether study of the pottery, which so far has only received a preliminary examination, will show any development of form or technique associated with the successive phases. Mud-brick was certainly employed in some of the periods. The bricks were of a form quite unlike those belonging to any other period, being hand-made and bun-shaped. None was certainly in position. The majority were found in tumbled debris, and those incorporated in walls were employed indiscriminately with stones, and appeared to be re-used material.

Beneath the layers containing Neolithic pottery and associated with structures of this type appeared buildings with mud-brick plastered walls and floors of the same character as those found at the north end of the Tell. As far as the evidence goes at present these were never associated with pottery. Since a change in structural methods is thus apparently associated with the appearance of pottery, it may be that this development was due to outside influence, and was not an indigenous invention. The buildings belonging to this pre-pottery phase so far uncovered in this area were less well-preserved than those of the same type at the north end of the Tell, and their details will therefore be described in connection with the work on that site. The only new structural feature observed here was a wall built mainly of mud-brick, but incorporating some stone, which contained a series of sockets for substantial timbers.

Clearance of the pre-pottery levels has only just begun over most of the site in the centre of the Tell. In a small area (Site G 2 on plan), however, linking the main site with the trench through the defences down the west side of the Tell, excavation was carried deeper. There, beneath two well-defined layers containing Neolithic pottery, the top of a very substantial wall began to appear. Its face was cleared to its foundations, which were found to be cut into a layer .50 m. above bedrock. Two well-defined surfaces were traced underlying the wall. Against its face was a fill in at least two stages, with associated surfaces, which contained flint implements, but no pottery.

The face of the wall is built of undressed stones, in somewhat irregular courses (PLATE I). Those in the uppermost courses are of medium sizes, but those in the lower courses, not clearly visible in the photograph, since they slope somewhat back, are enormous. The stone on which the hand of the man at the bottom of the photograph is resting is 1.5 m. long and 1 m. high. The upper courses are built on a batter. Excavation has not been carried down at the rear of the wall, but it appears to be built against an earth backing.

This wall is clearly not of a domestic nature, and has in fact every appearance of being a town wall. This is the outstanding discovery of the season. A town wall of this very early stage is at present indeed unique. Its stratigraphical position is absolutely secure (FIG. 2). The levels beneath it and associated with it are pre-pottery, while two deep later fills against its face are still pre-pottery. Over its top are two levels with Neolithic pottery. These in turn are overlain by the long succession of Bronze Age defences of the city, the earliest on a line just slightly in advance of the Neolithic one. On this line there are five successive walls (wall 5 is not visible in the section, but is seen on the south side of the trench, immediately overlying wall 4). The earliest, the destruction of which by an earthquake is strongly suggested in another section, can be dated to E.B.Ib, *c.* 3000 B.C. Wall 6 is set 5 m. further down the slope. Wall 7, superimposed on wall 6, was destroyed by intrusive EB–MB people, with their distinctive pottery and burials, probably about 2100 B.C. Their wall patching the front of wall 7, and the accompanying occupation, was buried beneath the thick make-up of the plastered glacis[3] which is the earliest of three successive Middle Bronze Age defences, dating approximately between 1900 and 1600 B.C., the latest of which is the great stone-lined fosse traced in the earlier excavations. The Neolithic wall can therefore be pushed well back in the fourth millennium, if not earlier.

Neolithic Jericho was thus an extensive settlement and moreover was dignified by an enclosing wall. It can thus lay claim to the title of town, probably the earliest in the world. It is a revolutionary idea in our knowledge of the development of settled occupation that the stage of community organisation involved in the construction of a town wall was reached before the invention of pottery.

Further work is required before the relation of the type of buildings with plastered mud-brick walls and floors to the town wall is established. As already mentioned, these were found best preserved in the site at the north end of the Tell. Excavation here started from Professor Garstang's level XI, a level in which he traced a building of megaron type. Two earlier layers were cleared, of which XII is visible in PLATE IIa, and XIII in PLATE IIb. Though each level is independent, and almost all the walls

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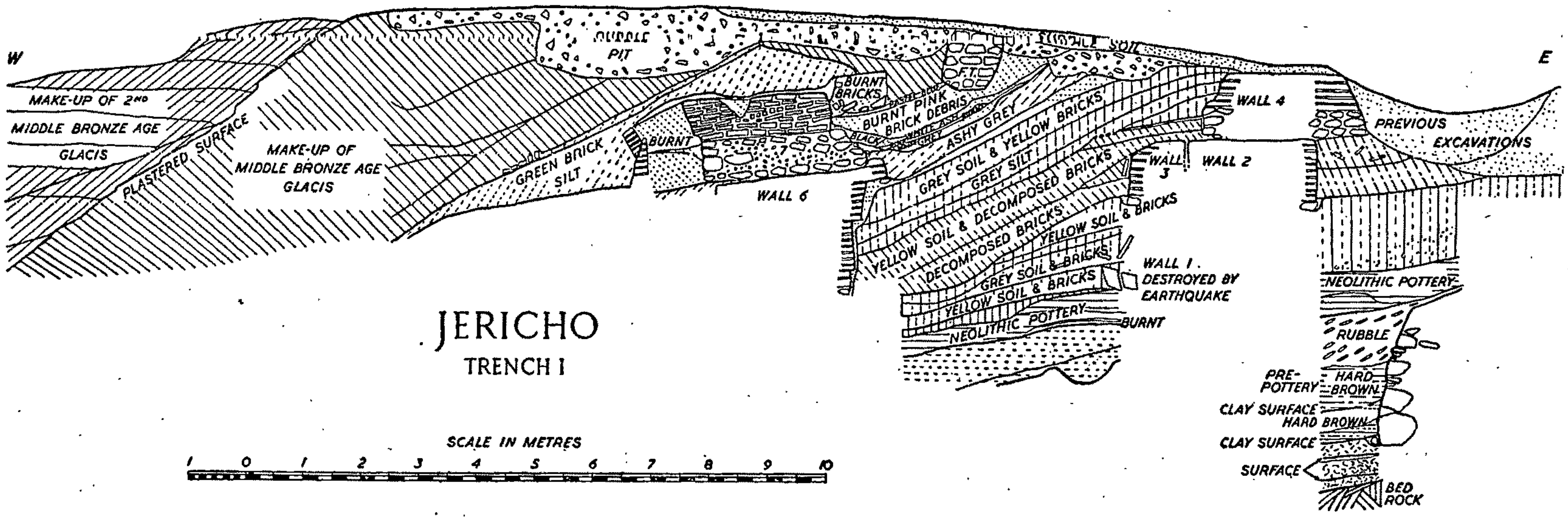
[3] More properly a scarp in military terminology, but the term glacis has become so well-established in Palestinian archaeology as describing this type of sloping defence, that it is probably less confusing to continue to use it.



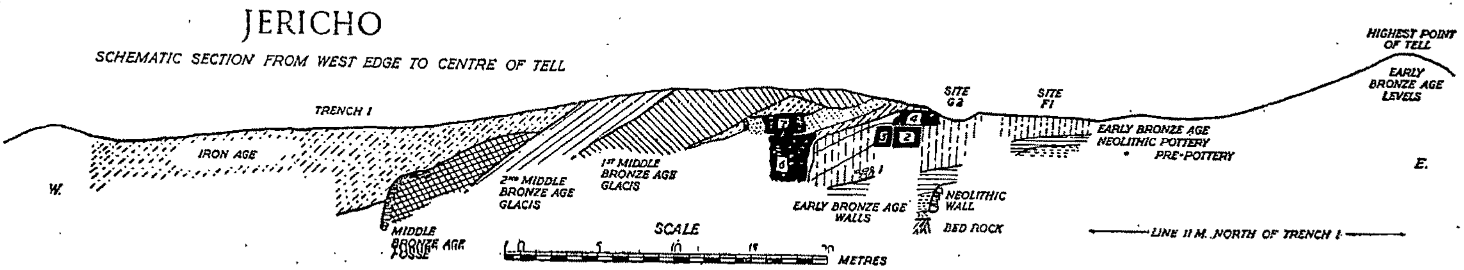


FIG. 2 *above*, SECTION OF EASTERN END OF TRENCH 1; *below*, SCHEMATIC SECTION TO SHOW RELATION OF UPPER SECTION TO WEST SIDE OF TELL belong to one period only, the same general plan is preserved. In each of them, the main room uncovered is connected with rooms to the east and west by wide doorways flanked by walls with rounded ends. In level XI, the room to the west was the inner room of the Megaron. The walls are straight and solidly built, the rooms approximately rectangular, with rounded angles. Both walls and floors are covered with a fine plaster and highly burnished.

Level XII has several interesting features. To the south of the main room was a small one, not part of the original layout. In the wall at the west end was a small niche, with a rough stone set as a pedestal at its base. In the debris not far away was found a remarkable stone, which exactly fitted the niche and in PLATE IIIa is shown standing in it. The stone is foreign to the immediate neighbourhood, and of volcanic origin. It is pointed oval in section, and is very carefully chipped all over to this shape. The evidence is very suggestive that it was a cult object, and that the room was a small shrine.

Another find possibly of ritual significance is that of the skull of an old man, lacking the lower jaw or any other bones, carefully set upright in the angle of two walls, and sealed above by the contemporary floor (PLATE IVa; the floor is visible on the left). Perhaps the inhabitants sought to preserve to the household the wisdom of this venerable individual. Two infants were buried in an adjoining room of the same house, and one beneath the floor of level XI.

The walls were very solidly built of mud-brick with mud-mortar of the same consistency. In most cases the bricks were slab-bricks of somewhat irregular size. But in some walls, bricks of a very unusual form were employed (PLATE IVb). They are long and cigar-shaped, somewhat flattened top and bottom. On top they are keyed to take the mortar with a series of impressions of the brickmaker's two thumbs arranged in a herringbone pattern. They were set in a particularly hard mud-mortar. Thumb prints are known on bricks at Teleilat Ghassul, but they are irregular and sparse, and in no way resemble these.

The rooms of each level were found filled with the debris of the superstructure of the walls. There were no signs of violent destruction in any of the levels so far examined. Presumably, when a house became dilapidated, it was pulled down, the debris levelled over, and a new house built on top. Material finds were reasonably numerous. They included a considerable number of worked flints, not yet examined. Of special interest were two small bowls of polished stone. A considerable number of animal bones were found, not yet identified. There were also several fragments of small querns. Some clay-lined bins set in the floor are shown in PLATE IIIb while in the corner of the same room was a hearth. A considerable amount of carbonised material was collected from this area and other parts of the filling, which it is hoped will enable a Carbon 14 dating to be given to the deposits.

Recent explorations have added considerably to our knowledge of the early stages of settled occupation in the Near East, but so far connecting links are lacking. No site closely parallels another, though similarities in technological development in some cases suggest rough contemporaneity. Such technological similarities suggest comparison between Jericho and Jarmo, in the foothills of north-eastern Iraq.[4] The mound of Jarmo was some 7 m. deep at the centre, and fifteen levels have been identified. In the top third of these, pottery is present, but apart from that the assemblage differs little throughout the occupation. The houses were of pisé, contained up to six rooms, and were rectilinear. There is evidence of agriculture, much grain has been found, and the

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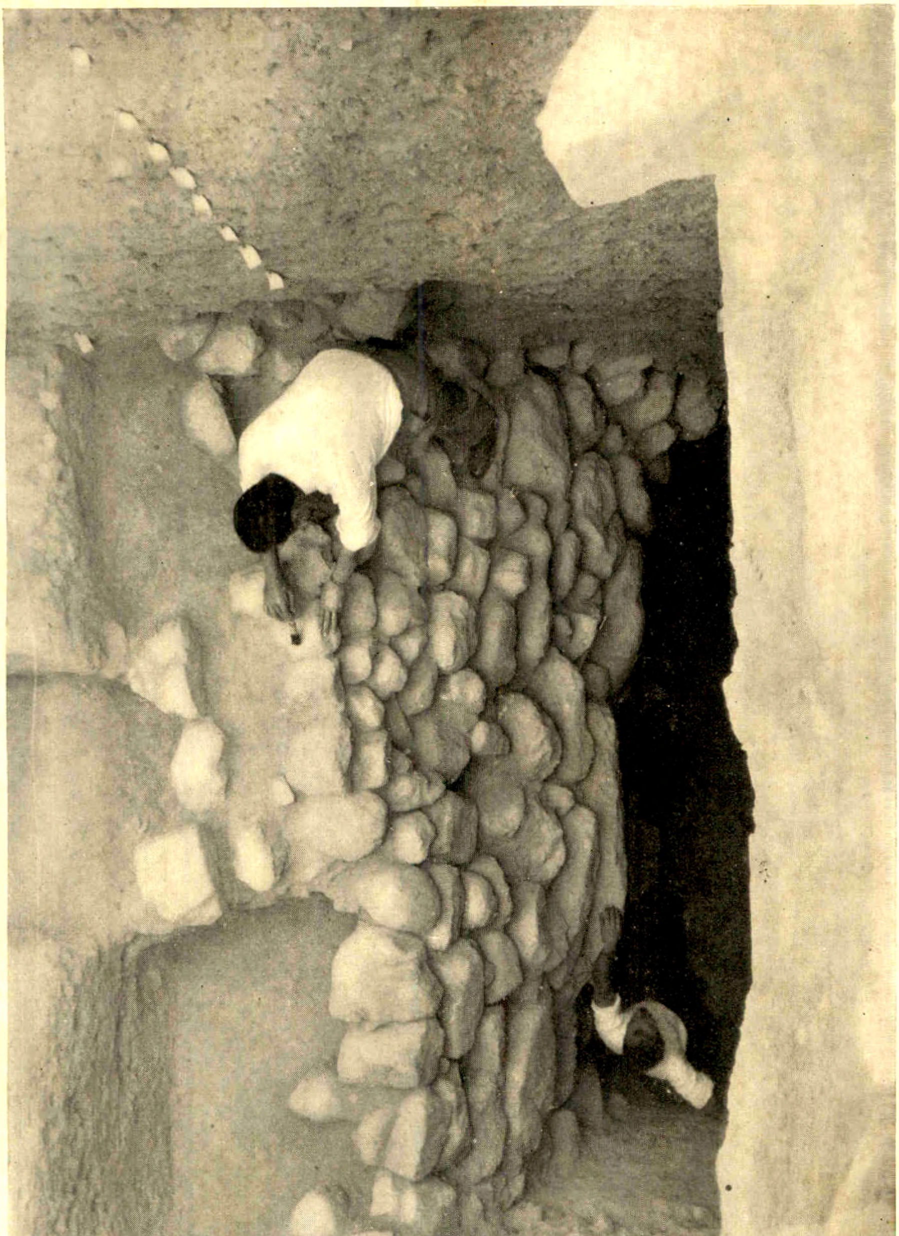
[4] ANTIQUITY, XXIV; *Sumer*, VII.

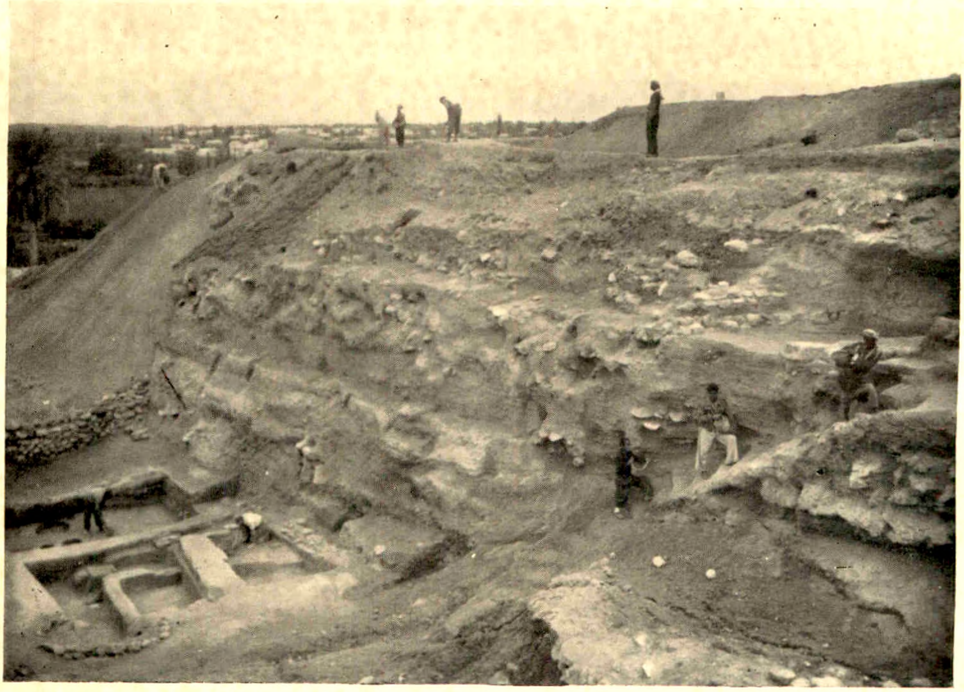
PLATE I



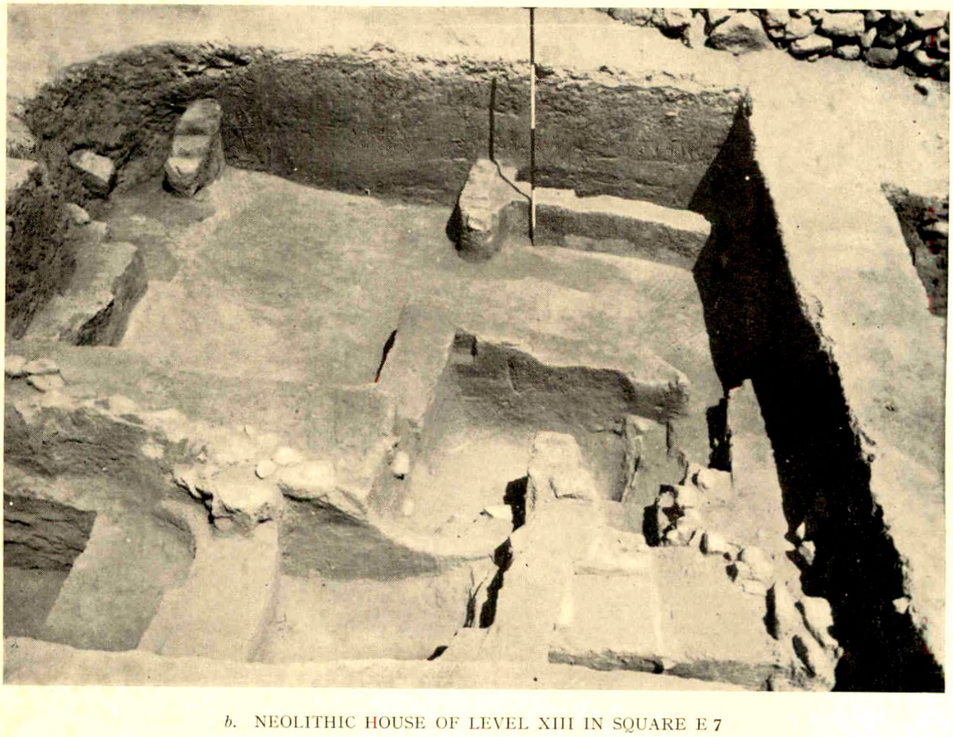
NEOLITHIC TOWN WALL

facing p. 120

PLATE II

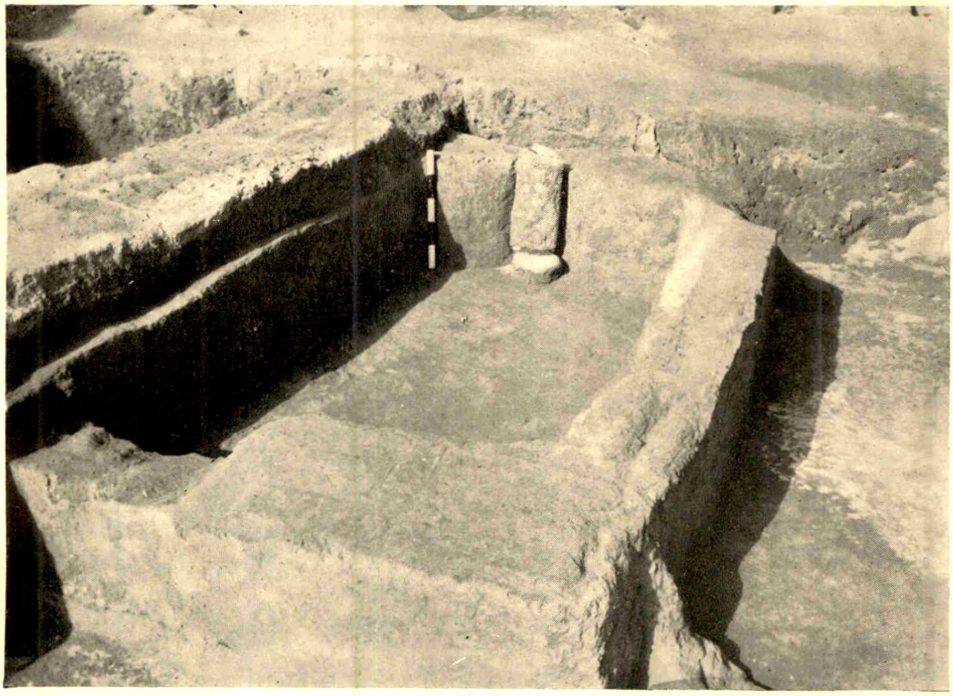


a. NEOLITHIC HOUSES AT BASE OF DEEP TRENCH IN SQUARE E 7

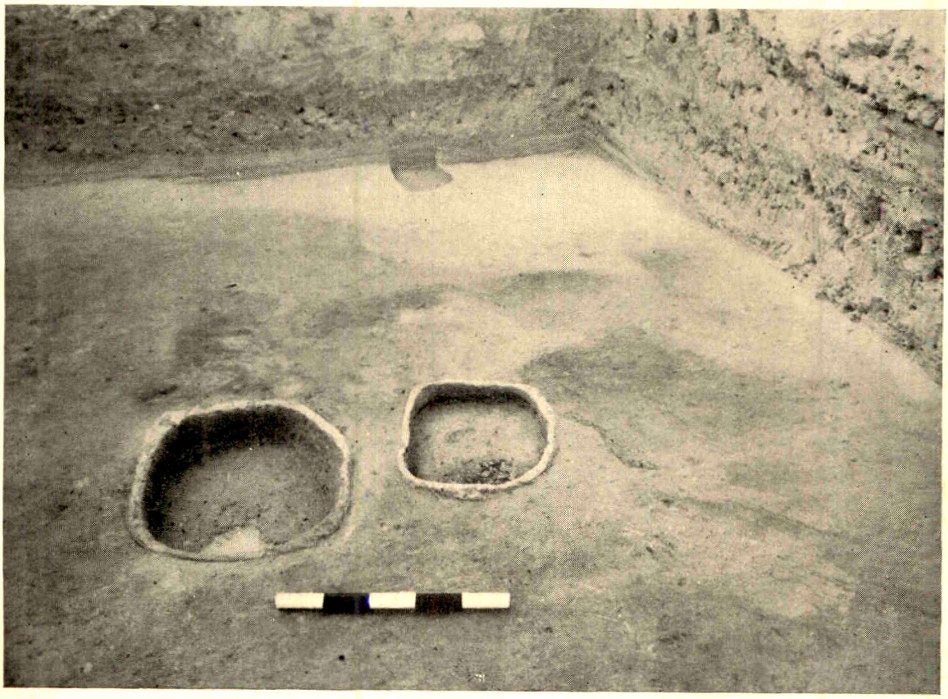


b. NEOLITHIC HOUSE OF LEVEL XIII IN SQUARE E 7

PLATE III

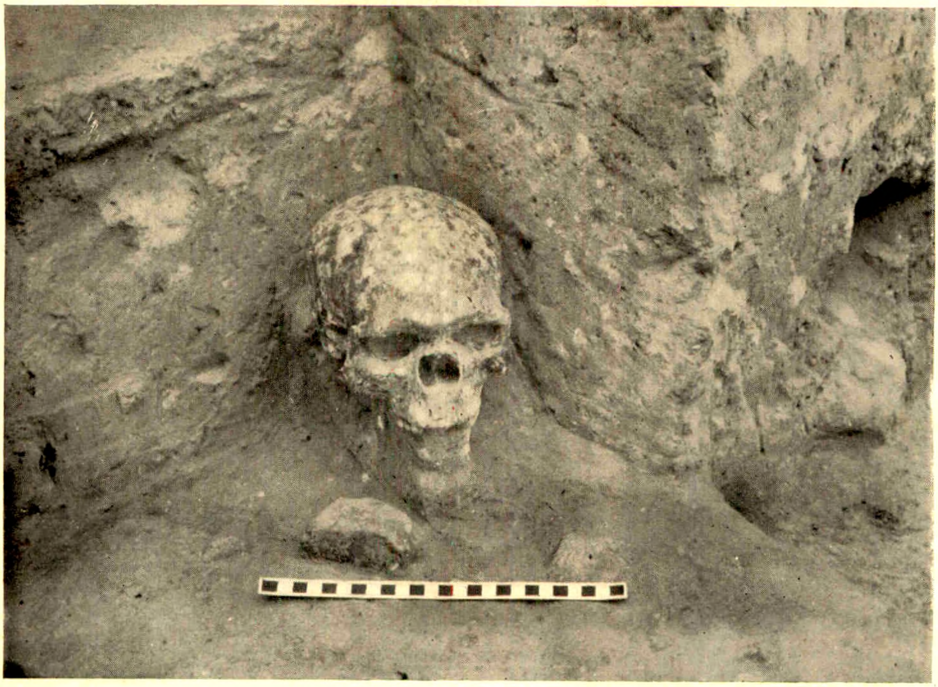


a. SHRINE IN SITE E 2, SQUARE E 7

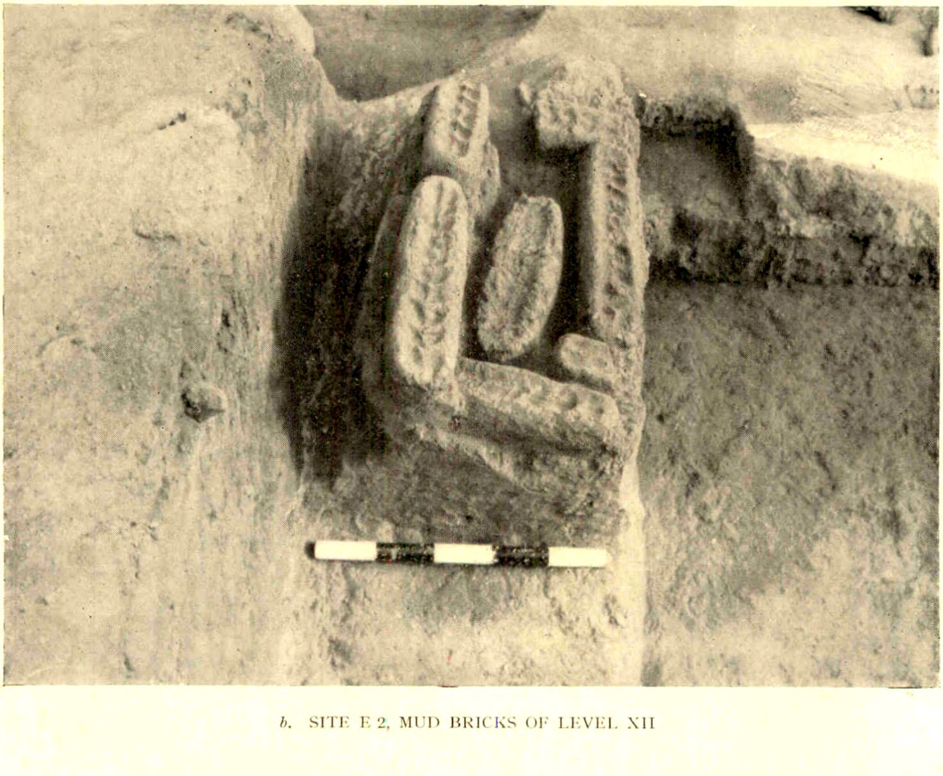


b. SITE E 1, STORAGE BINS

PLATE IV



a. SITE E 2, SKULL BENEATH FLOOR OF LEVEL XII



b. SITE E 2, MUD BRICKS OF LEVEL XII

bones of animals which were domesticable if not domesticated. The stages of development thus seem to be similar to those at Jericho. The pottery, however, would appear to be considerably rougher than that at Jericho, and neither that nor the flint equipment suggest any direct connection. In technological and economic development, Jarmo is preceded (though probably not immediately) by another site in the same neighbourhood, Karim Shahir, which is an open occupation site not yet developed into a village, with a Mesolithic equipment of late appearance, and suggestions of incipient agriculture. Further work alone can show whether the levels beneath the Neolithic deposits at Jericho represent a similar stage.

Professor Braidwood has suggested a succession of stages for the Iraq material, which may give some indication of chronology: (1) Karim Shahir: open site; (2) Jarmo: established village, during latter part of occupation of which pottery is developed; (3) Hassûna: early phase with crude painted pottery, later phase with Samarran painted pottery; (4) Halâf: elaborate early village material of the Chalcolithic stage; (5) 'Ubaid, etc.: town sites with temples. For the last-mentioned phase the currently accepted dating is *c.* 4000 to 3500 B.C. At Jarmo, a Carbon 14 test on snail shells, admittedly regarded as unreliable material, gives a date of between 5270 and 4630 B.C., which is a considerably lower date than had been expected.

One other parallel should be noted, and that is the occurrence in the lowest levels at Byblos of polished plaster floors.[5] These are the only ones so far known at all comparable to those at Jericho, but the resemblance is not very close, for the edges of the floors do no more than turn up against the base of the walls, which were in any case of a very flimsy nature. Moreover they are associated with sophisticated pottery with incised decoration, entirely unlike that of Jericho, which in any case does not occur with the floors. This phase at Byblos was assigned by M. Dunand to his Enéolithique A. By comparison with Tabbat al-Hamman[6] and Judeideh[7], M. Dunand places this phase somewhat earlier than the Tell Halâf stage. It is followed by the entirely different Enéolithique B, and the third Byblos stage with isolated rectangular houses, while the fourth stage, that of the first city and earliest Early Bronze level, is dated to the time of Jemdet Nasr, *c.* 3200–2800 B.C., by jar sealings.

Until the full succession at Jericho can be worked out, it cannot be said to what extent the stages at Jericho can be linked up with those suggested elsewhere. Palestinian archaeology has so far no firm connections with other areas prior to the Jemdet Nasr-Egyptian Late Predynastic stage, with which Early Bronze I can be equated. An excellent E.B.Ia tomb cleared this year shows that Jericho was occupied at that time. But how immediately that advanced city stage succeeded the preceding ones, the Chalcolithic of Professor Garstang's level VIII, and the two Neolithic phases which have been studied this year, is not yet clear. The occupation may in any case not have been continuous, for the links of level VIII with the nearby Teleilat Ghassul are very tenuous. M. Dunand's arguments[8] for a Bronze Age date for Ghassul are not convincing, for it has no valid contacts with the widespread Early Bronze Age of Palestine, and cannot have been contemporary with it. Ghassul may therefore fill in a gap when Jericho was unoccupied.

The 1952 season at Jericho has thus made most interesting discoveries and raised important problems. The discovery of a town wall at a stage which is at least technologically early may require a re-assessment of the rate of development of village communities from the food-gathering to the food-producing stages. With these problems in mind, it is hoped to carry out at least two further seasons' work, if sufficient funds are forthcoming.

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[5] *Bulletin du Musée de Beyrouth*, 1949–50.

[6] *Syria*, XXI.

[7] Braidwood, *Mounds in the Plain of Antioch.*

[8] *Bulletin du Musée de Beyrouth*, 1949–50.

Approx. Dates	Jericho		Egypt	Mesopotamia
	Mesolithic deposits	3 metres of deposits with microliths		
	Pre-pottery Neolithic	Extensive settlement with well-built houses Town wall		Jarmo: Lower ⅔rds.
	Neolithic with pottery	New structural methods (Possibly T. Ghassul comes here)		Jarmo: Upper ⅓rd. T. Halaf to *c.* 4000 B.C.
	Chalcolithic (or Late Neolithic)	Possibly restricted settlement Level VIII		Al 'Ubaid 4000–3500 B.C. Uruk 3500–3200 B.C.
3200	Early Bronze Age I	7 Successive City Walls	Late Predynastic	Jemdet Nasr 3200–2800 B.C.
3000	Early Bronze Age II		Beginning of Old Empire *c.*3100	
2600	Early Bronze Age III–IV		Pyramid Age to end of Old Empire 1st Intermediate	
2100	Intermediate Early Bronze—Middle Bronze	Destruction of latest E.B. city wall Low-grade culture	Middle Empire	
1900	Middle Bronze Age	Two successive plastered glacis and later Fosse	2nd Intermediate and Hyksos period	
1560	Late Bronze Age I	Site unoccupied	New Empire	
1400–1200	Late Bronze Age II	1400–1325 Occupation on east side of Tell traced in 1930–6 excavations None found in 1952		

NOTE.—No factual correlations exist prior to the Palestinian Early Bronze Age I.

Flint Tools and their makers

by HUMPHREY HUMPHREYS

[It should be observed that the Milankovich Chronology, the alternation of core and flake cultures, and the association of flake-tools with Neanderthaloid types and of bifaces with *Homo Sapiens* are all very much in question at the present time. But subject to this qualification, the figures here printed may give some sort of approximation to the length of the periods covered. EDITOR].

OUR knowledge of man's ancestry and of the early phases of his own existence can only be increased by new discoveries of flint tools and fossil man in association. Such finds have been very rare because industrial excavations like gravel pits, which might yield the evidence if carefully watched by skilled observers, do in fact destroy it. The unearthing of the Swanscombe skull in 1935 shows what might happen if the ability to recognise flint forms became more widespread and it therefore seems worth while to give a popular summary of our present knowledge.

The Pleistocene Period of Geological time—the period marked by successive ice-ages and their inter-glacial periods—is the stage on which the history of the hominids, the human family, has been set. In the last twenty years there has been much progress in our understanding of this period, and the appearance of an acceptable astronomical explanation for four recurrent glaciations has given us a gauge by means of which we can measure their absolute as well as their relative chronology. Croll, Ball and other 19th century astronomers had suggested that recurrent cold periods of climate might be occasioned by rhythmic changes in the earth's rotation and made mathematical estimates of their chronology : but these estimates could not be reconciled with the requirements of the geologist. Milankovitch has now worked out a course of calculations (1920–38) based on three related phenomena, the precession of the equinoxes, changes in the plane of the ecliptic and alterations in the eccentricity of the earth's orbit. These agree well with geological estimates based on quite different evidence and give us the time scale shown in Table 1.

TABLE 1

DIVISIONS OF THE PLEISTOCENE GEOLOGICAL PERIOD IN EUROPE

600,000 years ago :	Beginning of 1st (Gunz) glaciation.
550,000 „ „	Beginning of 1st (ante-penultimate) interglacial period.
500,000 „ „	Beginning of 2nd (Mindel) glaciation.
400,000 „ „	Beginning of 2nd (penultimate) interglacial period : The great interglacial.
250,000 „ „	Beginning of 3rd (Riss) glaciation.
200,000 „ „	Beginning of 3rd (last) interglacial period.
100,000 „ „	Beginning of 4th (Wurm) glaciation.
20,000 „ „	Beginning of Recent geological and climatic condition.

NOTE 1. The estimates of time are approximate and are given to the nearest 50,000.
„ 2. The proper names for the glaciations (shown in brackets) are those for the Alpine ice ages.
„ 3. The transitions from glacial to interglacial periods and *vice versa* were, of course, slow not sudden.
„ 4. Some of the ice ages, notably the 2nd and 4th, were partially interrupted by milder interstadial periods. See Table 3.

Into this frame-work flints and fossil men both have to be fitted, but a major difficulty at once arises. The evidence of glacial and interglacial conditions has mainly been obtained in Europe. Nearer the equator, although the snow-line on lofty mountains like Kilimanjaro came lower periodically, it was never cold enough for ice to produce the formations (boulder-clays, river terraces, raised beaches, etc.), which give us our geological evidence of ice action in northern regions. It is, therefore, not easy to compare the age of soils in Africa and Southern Asia with those of Europe, or to date accurately finds in the first two continents. Something, however, has been achieved. The essential condition for the establishment of an ice age—more important by far than a big drop in the mean temperature of the atmosphere—is a very long series of cool cloudy summers and wet winters. A big winter precipitation spells heavy snow falls in mountainous regions. If these are not melted by the summer sun they produce glaciers which, pushing ever farther down the valleys, cause a cold climate. When the glacier of the Rhône had its terminal moraine at Lyons there was an ice-age in central France. Now in Africa geologists find evidence of successive pluvial periods—epochs of heavy rain-fall—which have been correlated with the ice-ages of Europe and give us the means of cross-dating finds from the two continents. Unfortunately in S.E. Asia, where very important finds of fossil man have been made, geological evidence is both less complete and less conclusive in kind, and it is not yet possible to fit these finds accurately into the time-scale in Table 1.

Sticks and stones are the tools which Nature provides ready-made for our use and both are employed on occasions by monkeys as well as man. But the first hominids set out to *improve* these natural tools and with them they doubtless fashioned other fabrics, hides, horns and bones. All except the stones have perished and we are left to guess, from the shapes these were made to assume, the functions they fulfilled. Now of all the stones available to early man flint was by far the most useful. It occurs in chalk formations in lumps of a handy size, so that it does not need to be quarried; it is wide-spread over the surface of the globe; and it has the property, when struck by something hard such as another stone, of flaking in a plane controlled by the direction and delivery of the blow. This enables it to be wrought into definite shapes with sharp edges, for it is one of the hardest of natural substances. The detached flakes can be chipped into the shape desired (flake implements) or the central core from which they have been struck can itself be further fashioned into some useful form (core implements). Flint-knapping still survives in England for furnishing flint-locks to fire-arms. Where flints are absent, other hard stones like quartz, obsidian or quartzite, were shaped into tools by early man. Unfortunately S.E. Asia and Southern Africa, where some of the more important finds of fossil man have been made, are flintless areas, and comparison of the quartz tools of Pekin man with the European sequences of flint implements poses an anthropological problem not yet solved.

There are two outstanding features of flint implement distribution—the appearance of exactly similar types in all the main continents of the world, and their remarkable abundance compared with the very scanty remains of fossil man. The first feature seems to be evidence of endless migrations: for example, early Acheulian types occur in every continent except Australia. These were not movements of individuals but of groups, probably small family groups which almost certainly took many thousands of years to come about. Man made Acheulian flints for 300,000 years—half Pleistocene time—and in that period there were a number of severe oscillations of climate which must have affected his food supply and compelled him to shift 'to fresh woods and pastures new'. This vast expanse of time explains too the abundance of the fabricated flints we find.

If a tool broke or its edge became blunt it was a simple matter to make another. The modern knapper can strike off scores of gun-flints in an hour ; even the production of one or two flint tools a day if maintained for 100,000 years would yield enough to furnish every museum in the world with abundant specimens. It is probable that man was always rather a rare animal till he became a food-producer, and that was less than 10,000 years ago. The palaeolithic population of England in an Ice Age was possibly only a few hundred if we can judge from the demographic density of modern stone-age savages like the Tasmanians. Even so their remains are remarkably scarce as, in many soils, bones will perish in the course of time.

During the last twenty years an international nomenclature has found acceptance for the main types of flint implements, the names being derived from the locality (usually French or English) where the type was first recorded and described. These have also been dated to the extent that they have been fitted into the principal divisions of Pleistocene time and this is shown for the principal types in Table 2.

TABLE 2

RELATION OF FLINT FORMS TO CLIMATIC PHASES OF THE PLEISTOCENE IN EUROPE

Climatic Phase	*Flint Forms*
1st—(ante-penultimate) interglacial period.	Abbevillian : early Clactonian at end.
2nd—Glaciation.	None demonstrated.
2nd—(penultimate) interglacial period.	Late Clactonian followed by Acheulian.
3rd—Glaciation.	Levallois flakes early and late : some Acheulian forms persisting.
3rd—(last) interglacial period.	Levallois, Acheulian, Mousterian.
4th—Glaciation.	Mousterian followed by various blade industries. (see Table 3).

Before these are described it is necessary to consider the evidence on which a fabricated flint is distinguished from one shaped by natural forces. It has to be admitted that this evidence is often equivocal and falls far short of scientific proof. Any blow by another hard substance will chip a flint and such blows are often brought about by natural agencies. The pounding of pebbles by breakers on a beach, rapids rattling stones on a river bed, the intense pressures at the base of a glacier, even industrial processes such as the mixing in a cement works of chalk and its flint nodules with clay, all these have been observed to produce flints indistinguishable from many which have been confidently claimed as the works of man, such as the eoliths of Kent. But evidence even more conclusive against such claims is the fact that similar forms have been found in geological gravels dating not merely from the Pliocene, the period before the Pleistocene, but from the still older Miocene rocks laid down many millions of years ago. If there is one conclusion to which all authentic evidence points it is that true hominids are a product of the Pleistocene period—the last half million years. Flints found in association with the East Anglian Crags—geological formations which in that locality mark the transition from Pliocene to Pleistocene—have been claimed as man's handiwork and been given the local names of Norwician, Ipswician and Cromerian. Such claims cannot be unreservedly accepted until their association with fossil remains has been demonstrated.

The earliest flints which are universally admitted to be of human workmanship are the Abbevillian core implements known as hand-axes (FIG. 1). Even they have not been found with human remains, but their general form seems to be a clear expression of design, and they have not been discovered in Europe in soils earlier or later than the period of some 50–100,000 years, which spans their distribution. The evidence of distribution suggests that the Abbeville core industry developed in central Africa and spread early in the first (ante-penultimate) interglacial to Western Europe and Southern Asia. With the approach of the second ice-age it disappeared from Western Europe and was replaced by a new type of tool, Clactonian flake implements, though there was some overlapping of the two (FIG. 2). When the warmer conditions of the second interglacial had been established Clactonian flints (which have not yet been found outside Europe) gave way in their turn to Acheulian core implements (FIG. 3) which had probably been evolved in Africa from Abbevillian types and reappeared in Europe when the weather improved. As the cold climate of the third ice-age set in over Europe this story was repeated. Once again the Acheulian flints are found to fade away and be replaced by flake-tools formed by a new process known as the Levallois techique (FIG. 4). A large nodule was first reduced to the shape of a tortoise-shell, dome-shaped on one side and flat on the other ; a blow

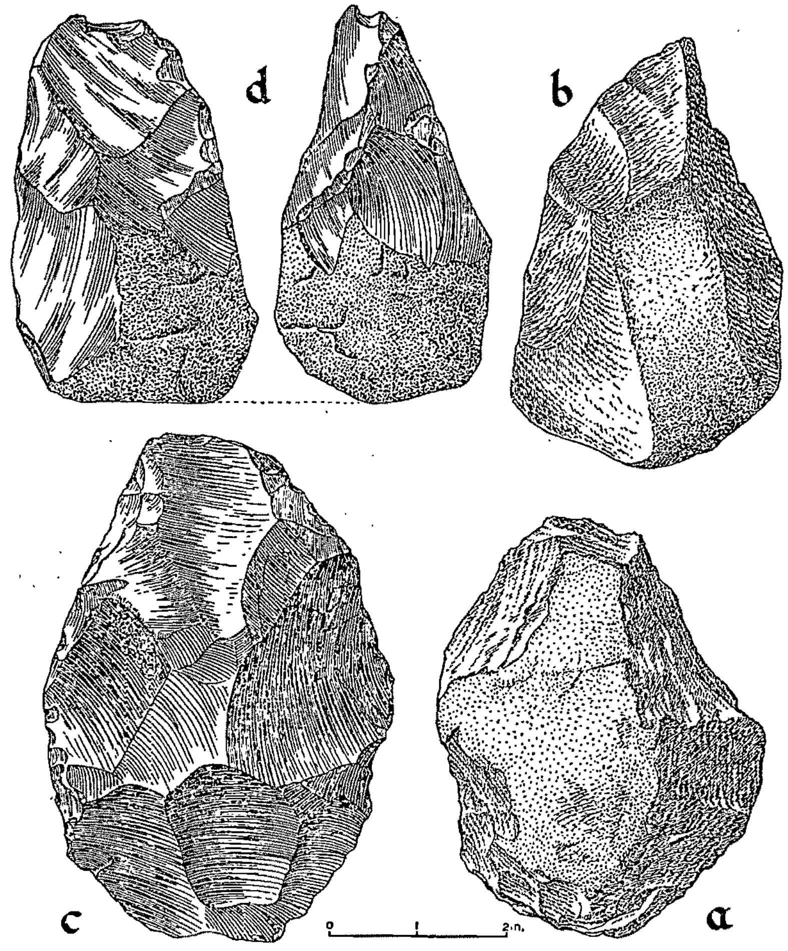

FIG. 1. ABBEVILLIAN TOOLS

a. Lava hand-axe, Tanganyika. *b.* Quartzite hand-axe, Morocco. *c.* Hand-axe, Chelles-sur-Marne. *d.* Hand-axe, 150 ft. terrace of Thames.

delivered on a prepared striking-platform at one end of the core, detached from the flat surface a single large flake which required hardly any retouching to make it a useful tool. The origin of this technique, an early attempt at mass-production, is still obscure. Levallois flakes are widely distributed in Africa and Europe and the art of making them may have originated separately in the two continents or spread from one to the other by migrations of the culture.

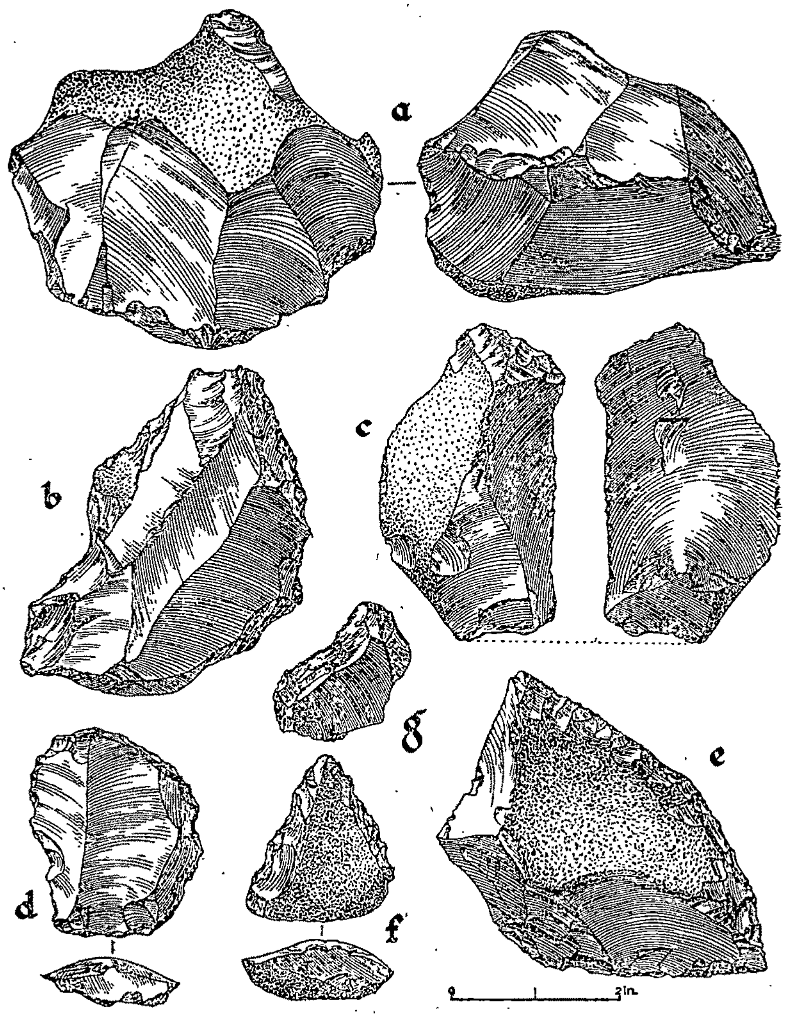

Fig. 2. CLACTONIAN ARTIFACTS

a. Clactonian flint core, and *b, c,* flake-tools, 100 ft. terrace, Swanscombe, Kent. *d.* Flake-tool, Clacton-on-Sea. *e.* Acheulo-Clactonian scraper, Suffolk. *f.* Proto-Mousterian flake-tool (Dordogne). *g.* Tayacian flake (Dordogne).

This curious association in Europe of the two flake industries, Clactonian and Levalloisian, with the second and third ice-ages and the intrusion of the two core industries, Abbevillian and Acheulian, in the first and second interglacial period, has given rise to the view that these two types of industry are associated with two different species of hominid, one of whom was more adaptable to a cold climate and fabricated flake-tools, while the other using cores retired southwards into Africa each time the ice advanced only to reappear when it retreated. Now there is clear evidence that in the second half of the Pleistocene period there did exist two species of man both of which have left their remains in Europe, Neanderthal and modern man (*homo sapiens*). The characteristic tools of the former are the Mousterian flakes found with his remains in deposits from the latter part of the last interglacial period. Typologically they appear to derive from Clactonian flakes and it so happens that from a late deposit of the first interglacial age laid down at a time when the Clactonian culture was spreading over Europe, there was recovered many years ago a lower jaw—the Heidelberg mandible—which closely resembles the mandible of Neanderthal man. The earliest remnant that

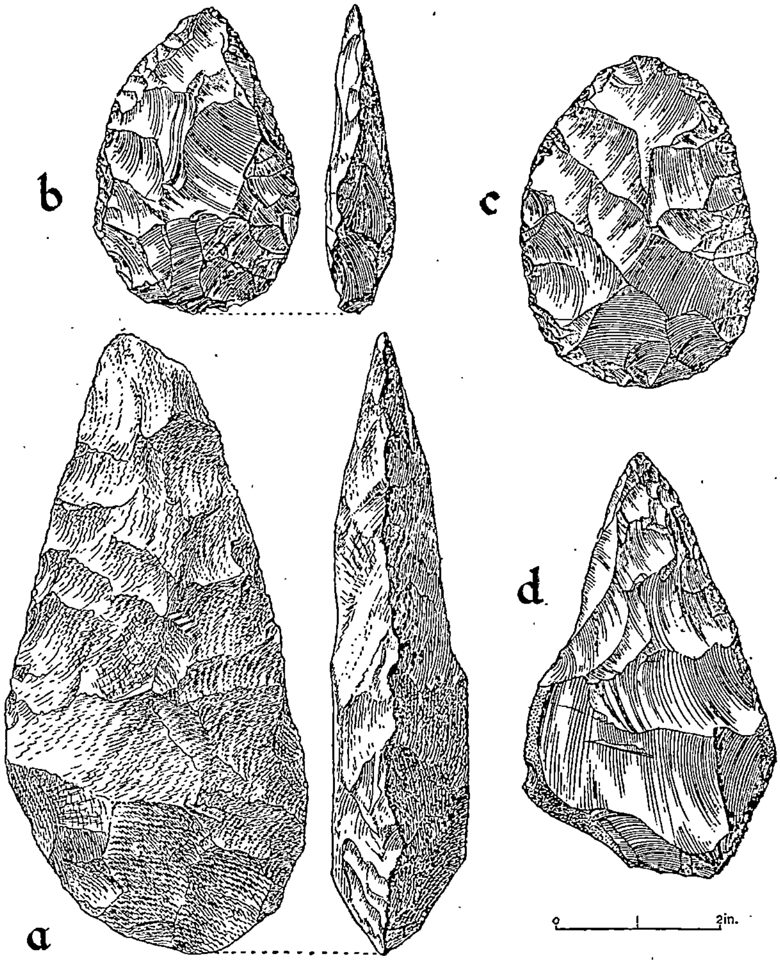

FIG. 3. ACHEULIAN TOOLS
a. Lava hand-axe, Kenya. *b*. Ovate, St. Acheul, near Amiens. *c*. Hand-axe, Palestine.
d. Hand-axe, of Micoquian type, Suffolk.

can be attributed to *Homo Sapiens* is the Swanscombe skull dated securely to the second (penultimate) interglacial and in the same gravel bed were found Acheulian tools. There is thus some evidence for the hypothesis that Neanderthal man—in the main a Eurasiatic species—was by predilection a flake-tool fabricator and took possession of Europe during

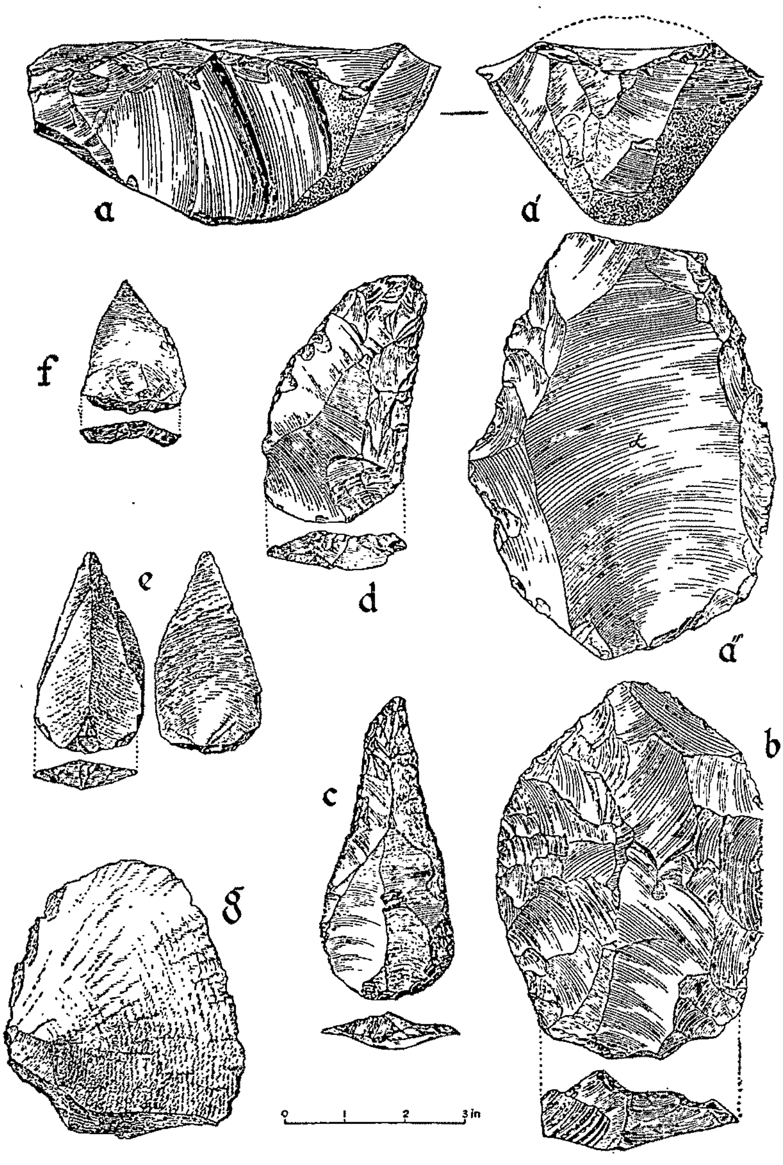

Fig. 4. LEVALLOISIAN AND LEVALLOISOID ARTIFACTS

a, a^1, a^2. Tortoise-core (three views), Baker's Hole industry, Northfleet, Kent. *b*. Flake with faceted striking-platform, similar to the one struck from *a* of the above core; Northfleet, Kent. *c*. Pointed flake-tool from Crayford. *d*. Knife-like flake-tool from Montières (Somme) *e*. Pointed flake-tool (chert), Somaliland. *f*. Pointed flake-tool (quartzite), Swaziland. *g*. Levalloisoid flake (lava), Acheulian 'floor', Kenya.

the ice-ages, while *homo sapiens*—perhaps of African origin—early developed a core industry and preferred a warmer climate. But only further discoveries of flints and skeletal remains in association can finally confirm or confute this theory.

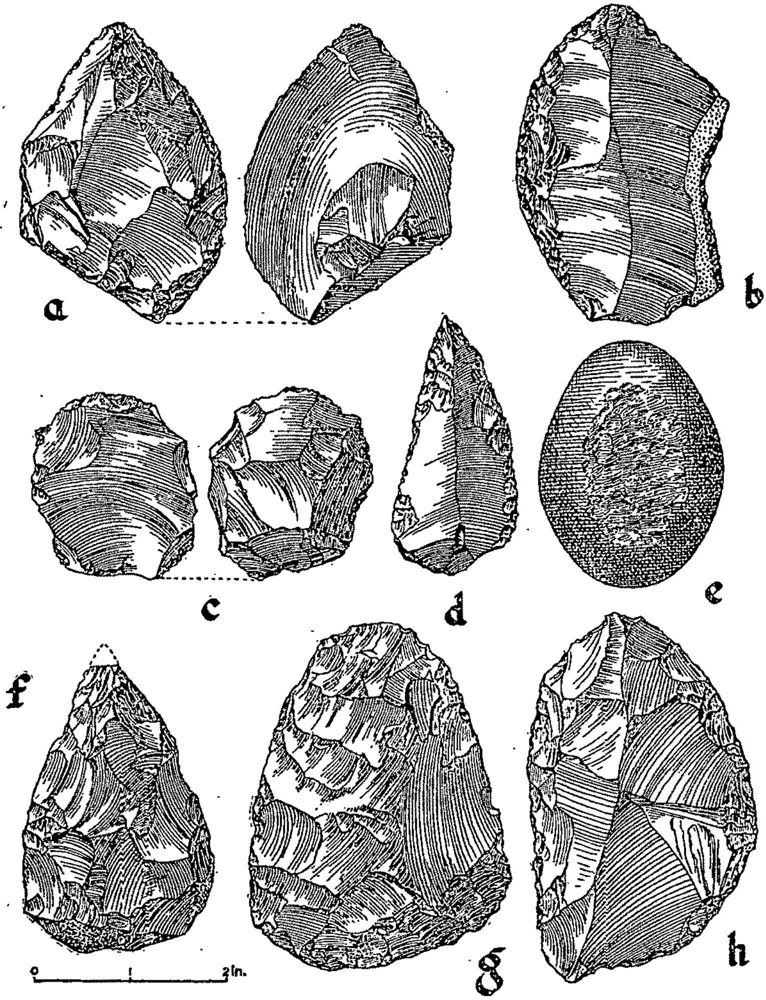

FIG. 5. MOUSTERIAN TOOLS

a. b. Side-scrapers, *c*, disc-core, and *d*, point, from rock-shelter at Le Moustier. *e.* Small anvil or hammerstone, Gibraltar caves. *f.* Hand-axe from Le Moustier. g. Hand-axe (chert), and *h*, oval flake-tool (flint), from Kent's Cavern, Torquay.

a-d. Typical Mousterian; *f-h.* Mousterian of Acheulian tradition.

Apart from the two just named, there is only one other hominid whose remains have been recovered in sufficient quantity to establish his separate existence as a species on a secure foundation. More than fifty years ago the skull of a single sub-man was discovered in Java, to which the name of *Pithecanthropus erectus* was given. In the last twenty years remains of more than a score of individuals have been unearthed in the Choukoutien caves near Pekin and several others in Java, and though the Chinese skulls were at first placed in a separate genus there is now general agreement among anatomists that the variations amongst these different individuals whether from China or Java are not wide enough to preclude the probability that they all belong to the same species. He had an average cranial capacity of about 1,000 c.c. compared with an average for apes of 500 c.c., for modern man of about 1,350 c.c. and for the recently extinct Tasmanians of about 1,200 c.c. He had mastered the use of fire, he successfully hunted wild deer and he fabricated tools of quartz which bear a general resemblance to Clactonian flint flakes. His culture cannot be correlated accurately with that of European flint fabricators though he seems to have lived at a level not lower than that of the Tasmanians. Geological evidence is as yet insufficient to give him his appropriate place in Table 1 though it indicates an early Pleistocene age.

To what uses were these early flint tools put? All we have to guide us in our guess work are experiments and our observations on the practice of modern savages still living in the stone age when first discovered. Thus the Tasmanians, now all extinct, lived a life so primitive that in spite of a climate with cold winters they had not learned to wear clothing or construct any kind of hut shelter. They made tools by chipping the local sandstone (flint is unknown in Tasmania) and these bore some general resemblance to Clactonian flakes. The chief use to which they were put was the chopping and scraping of wood to form their two hunting weapons, a spear and a club. Their wooden spears were first straightened by hand after being warmed in the fire: the end was then hardened by charring and sharpened by a stone scraper. Their users could aim them accurately enough to kill a kangaroo at 40 yards range. The shape of Clactonian flints suggests that they were used for similar purposes. At the type station of Clacton-on-Sea a yew spear was actually found in an interglacial peaty loam in association with a flint 'spokeshave'.

No modern savages have been observed using tools of Abbevillian type. The heavier ones could have served quite well as a hoe held in the hand for digging up edible roots and for smashing bones in quest of brain or marrow. The sharper and lighter Acheulian flints could have served for skinning game: but their distribution over all the main continents except Australia, their remarkable similarity everywhere, their persistence for nearly half the Pleistocene period—these facts suggest that they were a general-purpose tool, the jack-knife of primitive man. There is no evidence that they were hafted with a handle like neolithic stone axes. It is probable that a wooden spear like that of the Tasmanians was the principal hunting weapon of palaeolithic man. For big game like the mammoth or the aurochs—and there is ample evidence that he killed both in very considerable numbers—he perhaps employed pitfalls. His flint implements were, therefore, not weapons but tools.

We left the story of flint implement development with the onset of the third glaciation in which the Levallois flake culture predominated in Europe. It was still dominant when the last interglacial dawned, but Acheulian flints in their last (Micoquian) phase soon began to reappear. Many of them were now made from flakes not cores and it may well have been the interaction of these different techniques that produced in the latter part of the last interglacial the Mousterian flake culture of Neanderthal man (FIG. 5). It persisted through the first cold phase of the last ice-age at the end of which their culture and the men that made it were displaced by a totally new type of industry.

The last ice-age was a long one and lasted perhaps 100,000 years: it was marked by three stages of maximum cold separated by two milder interstadial phases.

TABLE 3

SUBDIVISIONS OF THE FOURTH (WURM) PLEISTOCENE GLACIATION IN EUROPE
100,000 B.C. to 20,000 B.C.

Climatic Phase	*Flint Forms*
1st—Cold Spell.	Mousterian with a few early Aurignacian blades.
1st—Milder Interstadial period.	Aurignacian.
2nd—Cold Spell.	Late Aurignacian. In France intrusion of Solutrean blades.
2nd—Milder Interstadial period.	Magdalenian in France. Creswellian in England.
3rd—Cold Spell.	Magdalenian in France. Creswellian in England.

NOTE 1. Early Aurignacian blades are often called Chatelperronian and late Aurignacian blades Gravettian.

„ 2. Solutrean blades show fine pressure flaking of the edges : the technique invaded France from the East.

„ 3. Typical Magdalenian blades are not found in England where a local Aurignacian technique persisted known as Creswellian.

„ 4. This time scale is based on astronomical calculations. Other evidence, e.g. radiocarbon dating suggests that some reduction of these estimates may have to be made, though the terminal date is much the same as that deduced from the evidence of varved clays in Scandinavia.

It was during the first of these milder periods that a race of *homo sapiens* invaded Europe and caused the final disappearance of Neanderthal man. Excavations have shown that he came from Western Asia and brought with him a new type of tool, the blade culture which consisted of narrow parallel-sided flakes shaped into a wide variety of specialised tools. These were the originals of the tools we still employ (FIG. 6). In some the blade was flaked obliquely at one end to form a narrow chisel edge, the burin. In others one edge was left sharp while the parallel edge was chipped or filed into a flat surface for the finger, thus producing the blunt-backed knife we still use (FIG. 7). Others were clearly designed for scraping hides, others were saws. Most of these flints were quite small and it is probable that many of them were mounted in handles of bone or wood. They were used for working bone, antler, mammoth ivory, and no doubt wood also, substances which in their turn became tools such as pins, awls and tips for spears These were often decorated by carving: art had dawned. This new culture is known as Aurignacian and was brought by a tall race of high cranial capacity.

In the long expanse of the last ice-age after the arrival of the Aurignacians, other versions of the blade culture succeeded one another in Western Europe. They have been given different names by the archaeologist and were probably developed by different immigrant tribes. Table 3 shows the principal cultures which are classed together as upper palaeolithic. Their Art often reached an incredibly high standard and from it we can make some deductions about their mental life. They believed in survival after death for they buried their dead with weapons and food for the next world. They practised magic of many kinds and understood the use of fire. They were skilled hunters and slew reindeer, the wild horse, the aurochs, rhinoceros, mammoth and the cave bear. But their flint tools, though they showed variations which enable the expert to distinguish the different cultures, retained the same essential types which the Aurignacians had introduced. All these blade cultures were Eurasian in distribution and it was only at the very end of the last ice-age that they spread to North Africa to form the Capsian industry. Throughout this age the older stone implements of tropical Africa persisted there unchanged.

The final retreat of the ice, which began about 20,000 years ago and lasted for many millennia, produced new problems for palaeolithic man. The steppe and tundra of the European lowlands, on which he had hitherto hunted the wild horse, the reindeer and the mammoth, became converted into dense forest. The big game moved away and men no doubt migrated with it. Those who remained behind, much reduced in numbers, experienced a decline in their culture, camping by lake and sea shore and living on seals, birds, fish and molluscs. But the men of this ' mesolithic ' period can be credited with some important innovations. They invented the bow and produced flint arrow-heads

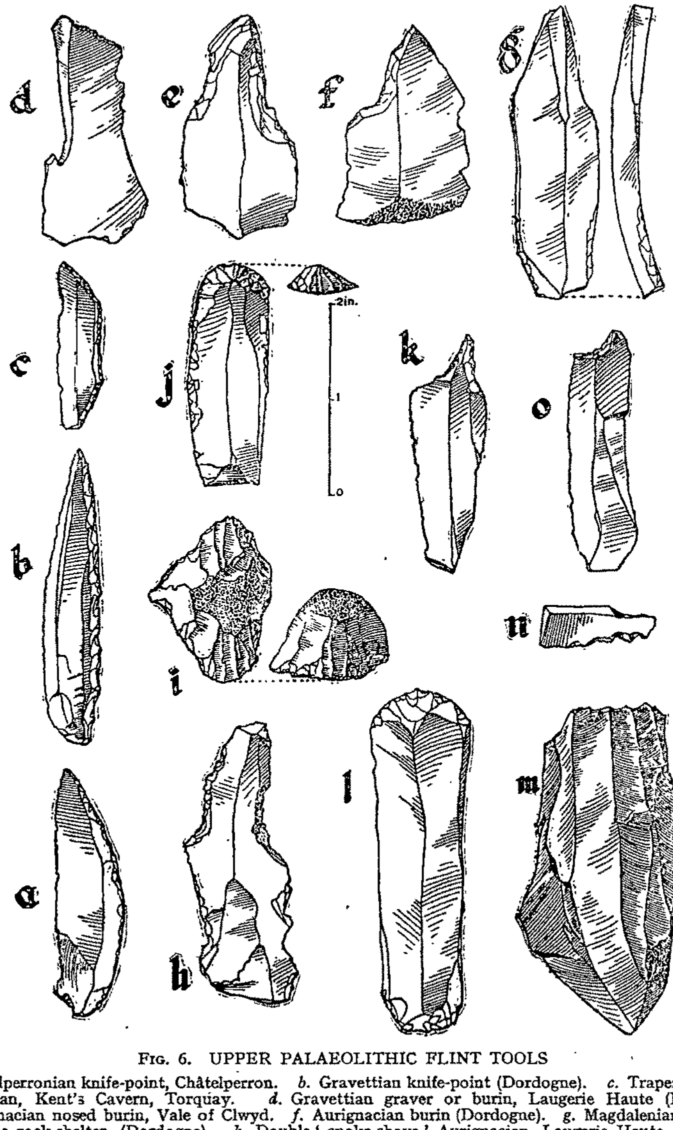

FIG. 6. UPPER PALAEOLITHIC FLINT TOOLS

a. Chatelperronian knife-point, Châtelperron. *b*. Gravettian knife-point (Dordogne). *c*. Trapezoid blade, Creswellian, Kent's Cavern, Torquay. *d*. Gravettian graver or burin, Laugerie Haute (Dordogne). *e*. Aurignacian nosed burin, Vale of Clwyd. *f*. Aurignacian burin (Dordogne). *g*. Magdalenian burin, La Madeleine rock-shelter, (Dordogne). *h*. Double ' spoke-shave ' Aurignacian, Laugerie Haute. *i*. Nosed scraper, Aurignacian, Laugerie Haute, *j*. End-Scraper, Vale of Clwyd. *k*. Solutrean 'hand-drill', Laugerie Haute. *l*. Double-ended scraper, Magdalenian (Dordogne). *m*. Magdalenian blade-core. *n*. Fragment of saw-blade, Magdalenian (Dordogne). *o*. Magdalenian concave-scraper or 'spoke-shave' (Dordogne).

for it; they hafted stone-axes with handles for chopping wood and began to grind their edges to make them sharp; they domesticated the dog; and they invented compound tools by mounting a number of very small flint flakes into handles or shafts. These 'microliths' provided barbs for arrows and fish harpoons, teeth for saws and, when agriculture began, blades for sickles. One way of doing this was to mount small flints in the tooth sockets of a large jaw such as that of an ox or horse with edges projecting to form a continuous composite blade: this made a sickle of the right shape. Such an implement has been found in Palestine and throws light on the legend of Samson slaying two hundred Philistines with the jaw-bone of an ass.

The climatic revolution which turned the open plains of Europe into forest changed the Sahara, which had previously been in great part grassland, into a desert, at first no doubt supplied with oases in which game-animals and men alike had to crowd together for the shrinking supplies of water. It was perhaps in this way that man learned first to capture the young, then to tame, then to breed the animals he had hitherto hunted: pastoralism had begun and agriculture accompanied it in the great valleys of Egypt and Irak. From this time onwards the North African coastlands, separated from Central Africa by the Sahara, became part of the Mediterranean cultural province.

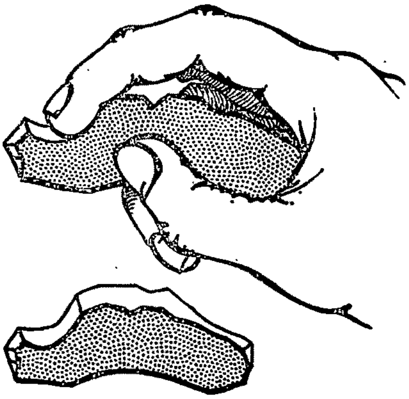

FIG. 7. AURIGNACIAN BLUNT-BACKED KNIFE WITH FINGER-REST SHOWING POSSIBLE GRIP

The advent of agriculture ushered in the Neolithic age and brought a need for new stone tools—the hoe, the adze, the stone quern as well as the sickle—but the need for hunting implements—knives and arrow-heads—remained. Neolithic flints are distinguished by fine pressure flaking and polishing and their beauty betrays an artistic pleasure in their production. Even after the introduction of metal, flint tools being so much cheaper continued in use, especially for arrow heads which are easily lost. It is not easy for us, living in an era of rapid change, to realise how slowly inventions spread in the ancient world. Agriculture was understood in Egypt and Irak by 5,000 B.C. but it was not till 2,000 years later that it was effectively adopted in Europe. Bronze was invented in Irak in the 4th millennium B.C., but did not reach Britain before 1750 B.C. Iron, smelted in Asia Minor in the 15th century B.C. took a thousand years to arrive in England. Even then flint tools were still employed and remained in use here till the Roman era.

NOTE.—Figures 1 to 6 are reproduced by permission of the Trustees of the British Museum (Natural History) and are taken from Dr K. P. Oakley's book *Man the Toolmaker*. All the tools figured are of flint unless otherwise stated.

The Oval House

by JAMES WALTON

THE place of the oval house in the evolutionary development of folk architecture is of particular interest. Although sporadic in occurrence it has been responsible for one of the principal present-day dwelling types, the hipped-roof. Innocent contends that the round house was the earliest and that the rectangular plan evolved through the intermediate oval stage which was produced by the joining of two circular frameworks. He further postulates that the ridge-pole was devised to connect two such frameworks.[1]

I have already indicated elsewhere that simple rectangular plans have been employed at least from the Iron Age and it is apparent that the rectangular gable-ended structure is certainly as primitive as the circular.[2] Some oval houses undoubtedly resulted from the union of two circular frameworks but the motive force behind this development was a desire on the part of peoples accustomed to building circular huts to emulate the rectangular type. Having seen the increased living space afforded by the rectangular plan, these peoples tried to copy it by joining two of their circular frameworks. This was not the only way in which oval houses originated.

Before discussing the various origins of the oval house it is essential to clarify the nomenclature employed to define the house types within this group. Åke Campbell's use of the term ' oval ' to describe an Irish house with a rectangular plan and rounded external ends has been criticized by Lochlainn[3] and Kissling has proposed the alternatives, ' ovate-oblong ' and ' rounded-rectangular ', although the type defined by these terms is not clear.[4] Under the heading of ' oval houses ' I have included all structures with rounded or partly rounded ends whose length is greater than their breadth. It is a loose grouping of houses with very different origins but archaeologists, ethnologists and travellers have used the term continually in this wide sense over a long period and any change would only result in confusion. What is required is a more scientific classification within the group itself and I suggest the following :—

1. ' Ovate-oblong ' houses. Those with true semi-circular ends and straight sides. Formed by the union of two circular huts.
2. ' Elongated circle ' houses. Those resulting from the elongation of a circular stone beehive hut or any other circular hut type.
3. ' Rounded-rectangular ' houses. Those with an approximately rectangular plan but with rounded corners or sometimes rounded ends. These may have originated in a number of ways.

Only circular huts with vertical walls can be joined together conveniently under one roof and consequently it is only among peoples building cone-on-cylinder or dome-on-cylinder huts that the ovate-oblong house has developed. In East Africa the Yao house is very often of this form as is indicated by Karl Weule's description. ' At Masasi my abode is a hut of the purest Yao style, 40 ft. by 20 ft. It is an oval structure whose roof is exactly like an overturned boat . . . The interior forms an undivided whole, only interrupted by the two posts standing as it were in the foci of the ellipse, and supporting the heavy thatched roof' (FIG. 1, 3).[5] This region is one where the rectangular hut type has pushed inwards from the coast into an area where the cone-on-cylinder *rondavel* was previously dominant. The fusion of the two has given rise to the ovate-oblong Yao house. Although it is not constructed as two separate circular frameworks yet its plan indicates that it originated as such. Its length is exactly twice its breadth and the roof-supports are placed at the centres of the two circles. The roof at its extremities consists of two half-cones, the forerunners of the hipped roof.

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[1] Innocent, C. F., *The Development of English Building Construction*, 1916, p. 8–11.

[2] Walton, James, ' Cruck-Framed Buildings in Yorkshire ', in *Yorks. Arch. Journ.*, vol. XXXVII, 1948, pp 49–66.

[3] Lochlainn, Colm, O., ' Folkliv, 1937, 2–3, 1938, 2 ', in *The Irish Book Lover*, January, 1940.

[4] Kissling, Werner, ' The Character and Purpose of the Hebridean Black House ', in *J.R.A.I.*, vol. LXXIII, 1943, p. 84.

In Basutoland ovate-oblong houses are found in plenty near Teyateyaneng in the north and in one or two scattered instances elsewhere.[6] The traditional Southern Sotho dwelling was an elaborate beehive hut, the *mohlongoa-fatse*, but this was largely replaced by the cone-on-cylinder *rondavel* introduced by later Bantu migrations from the north. These rondavels have a cylindrical wall of stone, turf or mud capped by a conical thatch. Still later Europeans brought the rectangular hut. Certain families then tried to adapt their circular building technique to a rectangular plan, so producing an ovate-oblong hut. This usually consists of two half-rondavels joined by straight sides (FIG. 1, 4, and PLATE Ib). Once again the ovate-oblong type resulted from adding straight sides to two circular structures placed together. The length of these huts is twice the breadth and the roof extremities are half-cones. In one instance, at Quthing, a woman had built two *rondavels* adjoining each other and she later connected them by straight side walls to produce a single hut. At Teyateyaneng, where the walls are of mud, one woman informed me that they built ovate-oblong huts because the local mud was not suitable for making sharp right-angled corners. In other words, the technical difficulties of building a rectangular hut were too great for those accustomed to building *rondavels* and the ovate-oblong hut was the compromise. Similar huts of recent date are found in the Transkei, where rectangular dwellings are associated with circular *rondavels*. It is significant that a ridge-tree, as such, is not employed in roofing these huts which tends to disprove that the ridge-pole originated in this way.

Sergei P. Tolstov has drawn attention to the existence of ovate-oblong houses amongst the Mangbattu whose traditional dwelling is a cone-on-cylinder hut but who have been influenced by the advance of the West African Negro rectangular house. Once again their attempts to emulate the rectangular plan have resulted in the ovate-oblong form. Similarly, in South America, 'the oval-shaped house is found amongst the Arawak-Yemanadis on the Amazon tributaries, amongst the Caraib-Bakairies of the upper Shingo—that is, amongst tribes living to the south of the chief region in which the rectangular "Maloca" occurs, and to the north of the region in which the round house predominates (the Matto Grosso and the Brazilian highlands)'.[7] This evidence was submitted to support the theory that the oval house has resulted from the fusion of two cultures, on which hypothesis he accounts for the oval Neolithic house type at Khwarizm.

In India, where the houses are predominantly rectangular, ovate-oblong huts are again found where pockets of cone-on-cylinder huts occur among the rectangular. Such a case was described by Stuart Piggott from the Deccan plateau, between Ajanta and

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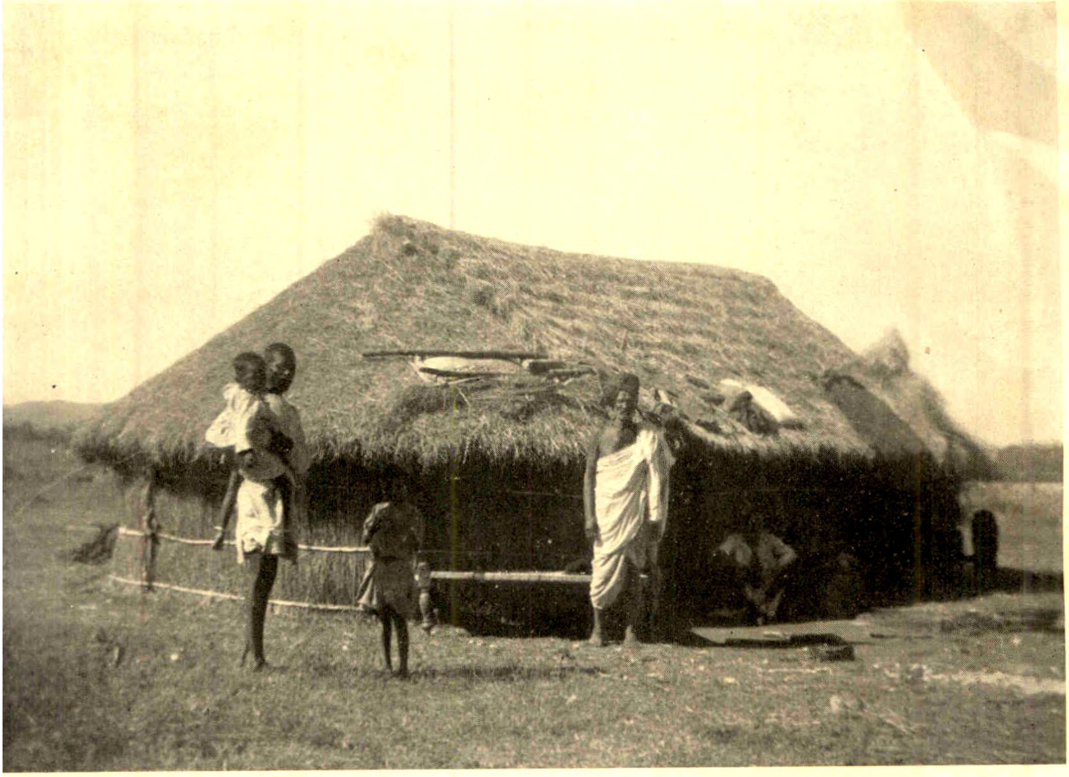
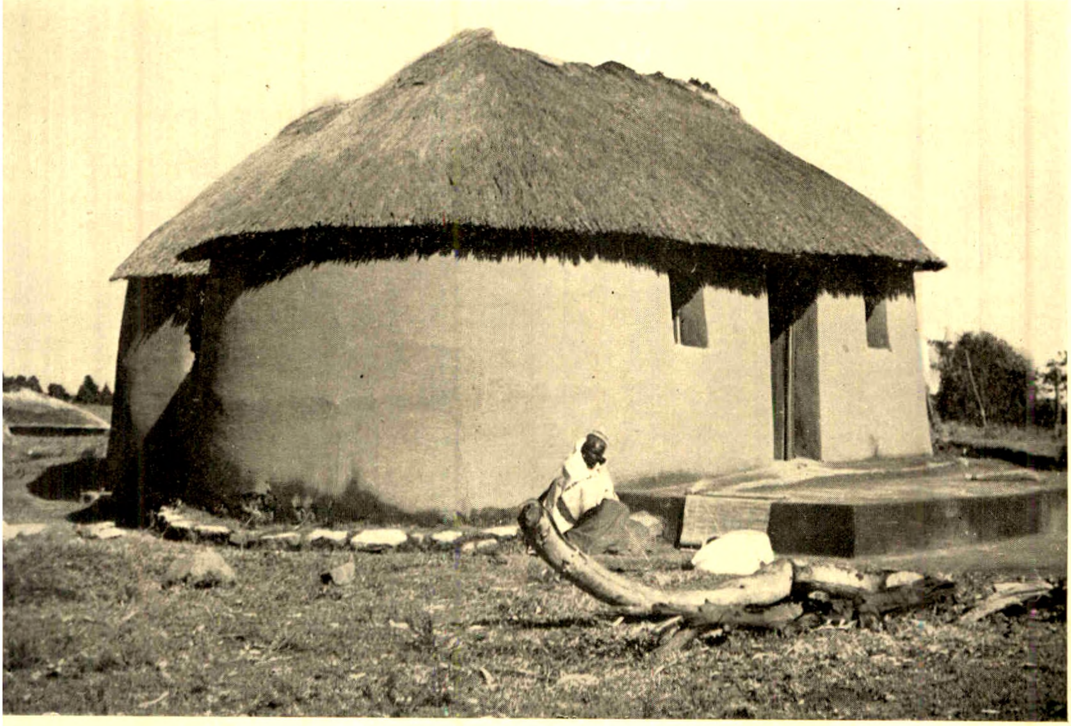
[5] Weule, Karl, *Native Life in East Africa*, 1909, p. 65.

[6] Walton, James, 'South African Peasant Architecture', in *African Studies*, vol. 7, 1948, pp. 139-45.

[7] Tolstov, Sergei P., 'The Early Culture of Khwarizm', in ANTIQUITY, 1946, p. 98.

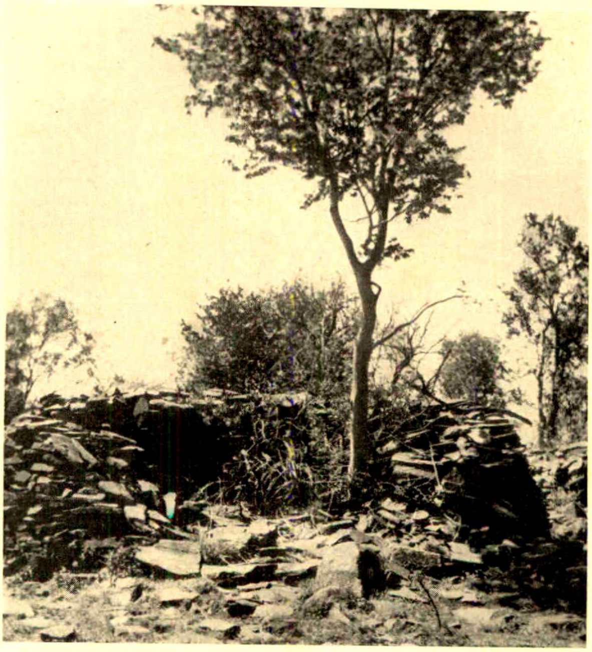


a. OVATE-OBLONG HOUSE, INDORE, CENTRAL INDIA
Ph. James Walton

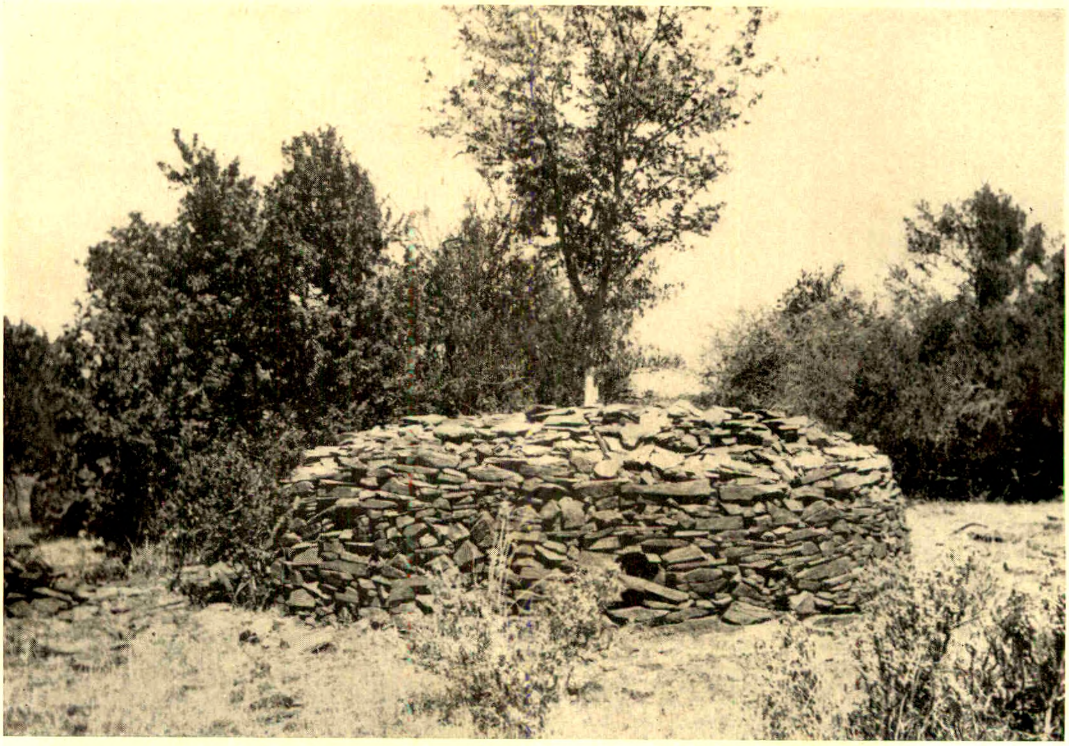


b. OVATE-OBLONG HOUSE, TEYATEYANENG, BASUTOLAND
Ph. James Walton

PLATE II



a. 'ELONGATED CIRCLE' HUT TYPE, DORINGBERG, ORANGE FREE STATE, SOUTH AFRICA—*front view*
Ph. James Walton



b 'ELONGATED CIRCLE' HUT TYPE, DORINGBERG, ORANGE FREE STATE, SOUTH AFRICA—*rear view*
Ph. James Walton

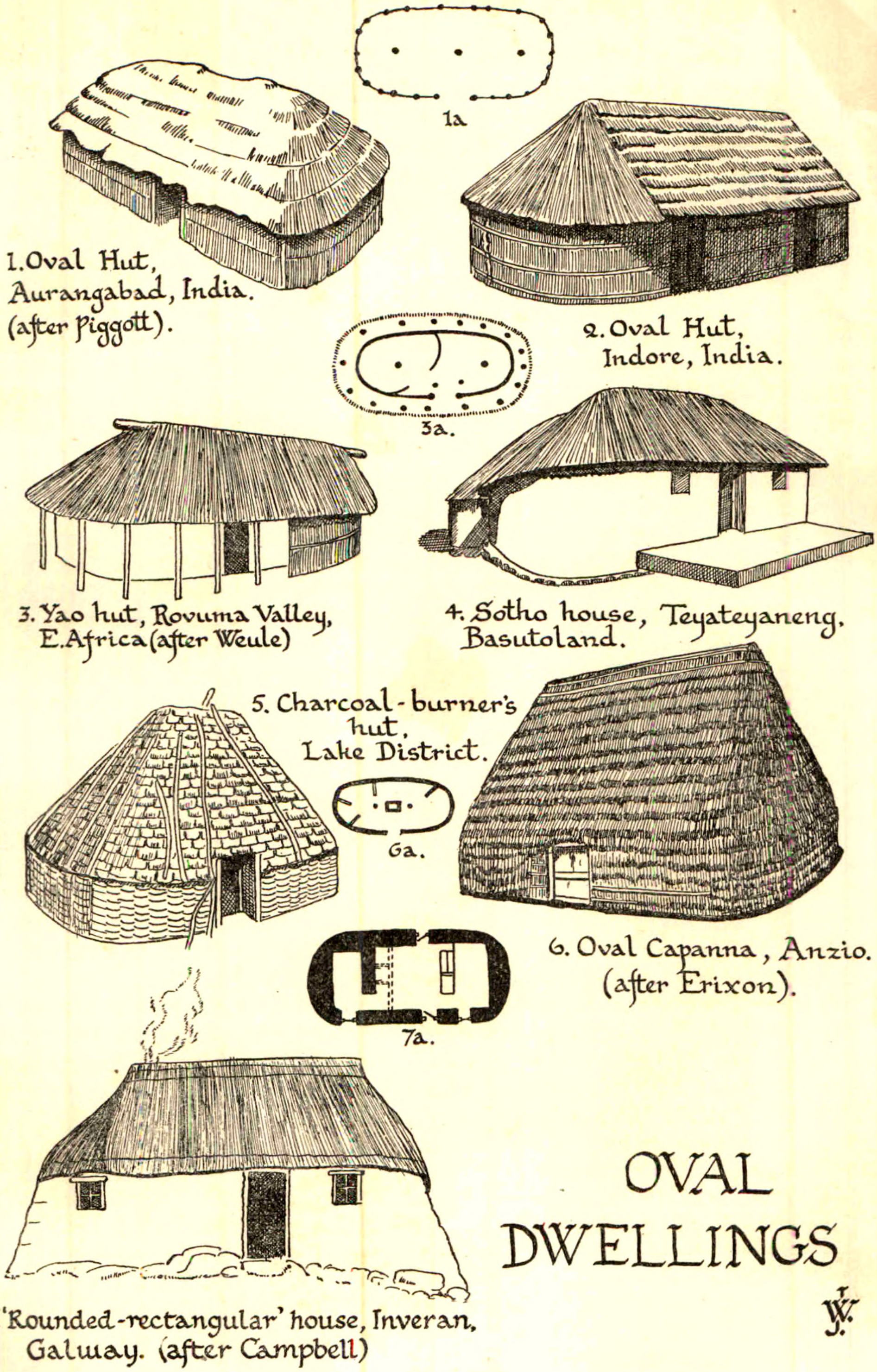

1a
1. Oval Hut, Aurangabad, India. (after Piggott).
2. Oval Hut, Indore, India.
3a.
3. Yao hut, Rovuma Valley, E. Africa (after Weule)
4. Sotho house, Teyateyaneng, Basutoland.
5. Charcoal-burner's hut, Lake District.
6a.
6. Oval Capanna, Anzio. (after Erixon).
7a.
7. 'Rounded-rectangular' house, Inveran, Galway. (after Campbell)
OVAL DWELLINGS

Fig. 1

Aurangabad, where ovate-oblong dwelling houses are associated with circular store huts.[8] These do not appear to represent a joining of two circular frameworks so much as an amalgamation between the prevailing rectangular type, with its central row of king-posts, and the cone-on-cylinder hut (FIG. 1 and 1, 2). Similar ovate-oblong huts have been noticed by the writer near Indore, especially among the low-caste tribes who often live in crude circular huts on the outskirts of a village in which all the other dwellings are rectangular (PLATE Ia and FIG. 1, 2).

In Europe, too, houses resulting from the union of two circular frameworks are not unknown. In the Roman Campagna the shepherds and charcoal-burners still erect their round and oval *capanna* (FIG. 1, 6 and 6a). ' To understand the construction it is necessary to know how the shepherds and charcoal-burners on the Campagna are accustomed to build their houses. The start is made by setting 3 or 4 supports in a pyramid and knotting them together at the top with bast rope. If a circular hut is to be made *one* such pyramid support is sufficient, under which the hearth will have its place, and the work proceeds by building the walls and completing the roof, which can usually be done by one man alone. When putting up an oval *capanna* the builders are not content with *one* but usually set up at suitable intervals 3 pyramids of the same kind as described above. A ridge beam is then laid on the forks formed by the support tops above the binding points. The two ends of this beam mark the outer ends of the roof ridge. The centre pyramid here also is placed over the fireplace and is perhaps really only a heritage from the circular hut. It is only in buildings of appreciable length that it has any real bearing function '.[9]

A somewhat similar hut was previously used by the charcoal burners of South Yorkshire and the Lake District (FIG. 1, 5). This was ' formed of two tripods, with the tops of the tripods connected by a ridge-pole, and with the door in the side. It was evidently formed by setting up the framework of two huts side by side, and then completing the construction to form one hut '.[10]

In the region around Alberobello, in Southern Italy, stone beehive-roofed dwellings, known as *trulli*, are still widely employed. These are often single structures consisting of a beehive-shaped stone roof capping low cylindrical or square walls. When more accommodation is required two or more are joined together, each room retaining its own separate roof and usually its own entrance. In some cases, however, two *trulli* have been joined together to form an ovate-oblong house covered by a common slate roof with rounded hipped ends.[11] It represents the latest *trulli* development.

From these descriptions it is apparent that the ovate-oblong form has resulted from the union of two dome-on-cylinder or cone-on-cylinder frameworks and represents an attempt on the part of people accustomed to building circular huts to construct a rectangular dwelling. It is invariably found where later rectangular plans have impinged on existing circular house types.

Just as the desire for increased living space prompted certain peoples to join two circular cone-on-cylinder huts together to form the ovate-oblong type so it urged the builders of stone beehive huts to develop the ' elongated circle ' house plan. ' The oblong form of building can obviously have been developed or been invented in a number of ways, as there are varying antecedent conditions in different cases. There is however one oval or oblong form of building which may have its origin in the stone-built domed-roofed beehive house. The construction with a domed roof does not permit of much widening of the space between the walls. Even if very long and suitable flag-stones are at hand, the limits of the cyclopean arch will make it impossible for the primitive builder to enlarge the circumference of his house to any great degree. To get more room he must put two or more circular beehive dwellings together, or he must go over to the oval or oblong form '.[12] In support of this theory Åke Campbell cites Clochán na Carraighe, on Inishmore Island, Aran, Co. Galway, as an example of this type.

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[8] Piggott, Stuart, ' Farmsteads in Central India ', in ANTIQUITY, 1945, pp. 154–6.

[9] Erixon, Sigurd, ' Some Primitive Constructions and Types of Lay-out, with Their Relation to European Building Practice ', in *Folkliv*, 1937, pp. 124–55.

[10] Innocent, C. F., op. cit., p. 12.

[11] *The National Geographic Magazine*, 1930, p. 254.

The association of elongated-circle and circular hut types in Anglesey and elsewhere supports this view. Excellent examples have recently been studied by the writer at Doringberg, in Orange Free State, South Africa.[13] Up to about 150 years ago this flat plateau was occupied by the Ligoja and Ba-Taung tribes who constructed extensive stone kraals and circular beehive huts which are still in an excellent state of preservation. The earliest huts are circular stone beehives with a maximum diameter of about 9 feet internally. When, later, more room was required it was found impossible to increase the span and so the builders elongated the circle to a length of 14 feet along one axis while the other axis remained the same (PLATE II).

This appears to be the normal method of enlarging the stone beehive hut except when it is first raised on vertical walls as at Alberobello. I cannot find any evidence of two or more stone beehive huts having been united under a common roof. Instances are by no means uncommon in the Hebrides when two beehive huts have been joined together but each retains its separate identity.

Stone houses which have a rounded-rectangular plan still exist in Ireland (FIG. 1, 7 and 7a), Scotland and Wales. They invariably have rounded hipped thatched roofs. The Hebridean black-houses were rounded-rectangular in plan : ' the walls, rounded at the corners, had a receding slope from the ground upwards ; their roofs had no perceptible ridge and did not reach the outer edges of the walls ; with their thatched gables. the roofs continued the rounded contour, so that the houses looked like mounds, and sometimes took on an elongated oval form '[14].

The houses of Achil were ' heaps of rude stones moulded by the tide, procured from the beach, uncemented ' ; they were ' rounded at the gables, and roofed with fern, heath and shingles fastened on by straw bands '.[15] According to Caesar Otway ' an Achil house consisted of a dry stone wall of a form like an obtuse oval, for they had not yet arrived at the art of making a square quoin, or erecting a gable end '.[16]

The origin of these rounded-rectangular houses is not so clearly defined as the two types already discussed and they probably have more than one source of origin. Otway suggests that the Achil houses were rounded because they had not yet arrived at the art of making a square quoin or erecting a gable end. Once again it is the possibility that people accustomed to building round houses were limited in technique when turning to the rectangular plan.

A second possibility is inferred by Kissling when he states that ' the rounded corners and, sometimes, end walls which give these houses a rounded-rectangular form, leave nothing on the outer surface to catch the wind. Whether or not this ' tendency to circularity ' is to be explained as a stage in development from an earlier type of dwelling in the islands, it is undoubtedly suited to the conditions in a windy district '.[17] Were the rounded corners built intentionally to withstand the Atlantic gales ?

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[12] Campbell, Åke, ' Notes on the Irish House ', in *Folkliv*, 1938, p. 180.

[13] A detailed survey of these sites is at present being made by the author.

[14] Kissling, Werner, op. cit., p. 82.

[15] Hall, Mr and Mrs S. C., *Ireland : Its Scenery, Character, etc.*, vol. III, 1843.

[16] Otway, Caesar, *A Tour in Connaught*, 1839.

Still more recently T. C. Lethbridge has linked the rounded-rectangular stone house with an upturned boat. ' It is not far from the currach or umiak, resting bottom upwards on a ring of stones, to the primitive oval hut with stone walls and a boat roof '.[18] Instances where a boat has been used as a hut roof do not support this thesis ; it appears rather to be a make-shift expedient. There may have been a parallel evolution between house roofs and skin-covered boats but proof is wanting that the one influenced the other. Oval forms in each case may have resulted from the elongation of the circular. Lethbridge does not submit any evidence in support of his supposition. He concludes, ' I think this is probably the way development came but I do not expect the theory to be accepted yet awhile. Too much ink has been expended to prove that the Hebridean Blackhouse is a long house from Norway or Denmark and all the rest of it. The Blackhouse is no such thing. For these long houses of Scandinavia and Denmark, which were later to be found in Iceland and Greenland also, were houses with aisles which the Blackhouse was not. The Blackhouse is a lengthened descendant of the Welsh Cytiau, or house of the form we excavated on Gateholm '.[19] Lethbridge appears to advance two different theories for the origin of the rounded-rectangular blackhouse—first, that it is modelled on the upturned boat, for which hypothesis there is at present little confirmation, and second, that it is the development of an elongated circular stone dwelling, for which there is considerable support.

In his refutation of the association of early basilical Scandinavian dwellings with the blackhouse I think he is certainly correct. The house which he illustrates from Gateholm is a central-passage house with a central hearth, a type which Iorwerth C. Peate has described from Wales[20] and Åke Campbell has noted in Ireland[21]. The blackhouse is certainly associated with the same group. Campbell has pointed out that the central hearth allowed for the hipped roof and he concludes that central hearth houses stand in close relationship to beehive houses of circular or sub-rectangular character.

The rounded-rectangular type may, then, have originated in one of a number of ways :—

1. As a rectangular dwelling the corners of which were rounded because of the limitations of the builders. This may have been due to the nature of the available materials which were unsuitable for making a square corner or it may have been due to the inability of the builders to construct either a gable or a square corner. Houses whose corners are rounded both internally and externally may have originated in this way.
2. As a rectangular dwelling whose corners were rounded to resist storms. Houses with right-angled interiors and rounded exteriors may belong to this class.
3. As a further development of the ' elongated circle ' type. Central hearth blackhouses and certain similar Irish houses probably originated in this way.
4. As a development from the upturned boat resting on a low wall of stones. This is a possibility but supporting evidence is lacking.

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[17] Kissling, Werner, op. cit., p. 78.

[18] Lethbridge, T. C., *Merlin's Island*, 1948, p. 81.

[19] Lethbridge, T. C., op. cit., p. 81.

[20] Peate, Iorwerth C., *The Welsh House*, 1944, pp. 51–84.

[21] Campbell, Åke, op. cit., p. 183.

Books Received

ANCIENT HISTORY, from prehistoric times to the death of Justinian, by C. A. ROBINSON, Professor of Classics, Brown University. The Macmillan Company, New York, 1951. £2 5*s*. [738 pages, 113 to human origins and the Most Ancient East, the rest to Greece and Rome : chronological tables, bibliography, 83 maps and diagrams, 138 illustrations. First part necessarily compressed and dates controversial ; but if supplemented (or substituted) by Childe's *Most Ancient East* (Routledge and Kegan Paul, 1952, 35*s*), gives good conspectus of ancient history, suitable for ANTIQUITY readers].

STUDIES PRESENTED TO DAVID MOORE ROBINSON on his 70th birthday, Vol. I : ed. by GEORGE E. MYLONAS : Washington Univ., 1951. $25. [Contains bibliography of D.M.R., the excavator of Olynthus, and sections on Prehistoric Greece, Egypt and the Far East, Architecture and Topography, Sculpture, Paintings and Mosaics, lists and index : in all 876 pages and 111 plates. Festschrifts defy summarization but are always, like this one, full of good things—Childe on Slings, Gjerstad on the Agger of Servius Tullius, L'Orange on the Oriental Ancestry of Trelleborg, the Viking round fort in Denmark, and much else].

PENGUINS AND PELICANS

THE ROMANS, by R. H. BARROW. (Pelican), 1949. 1*s* 6*d*. [Described as ' an analysis of Rome's achievement and of her contribution to the founding of Western Civilisation '].

GREEK TERRACOTTAS, by T. B. L. WEBSTER. (King Penguin), 1951. 3*s*. [An authoritative and badly needed account of these charming figures : dates from 7th to 1st century B.C. : 54 excellent plates].

THE CROWN JEWELS, by OLIVER WARNER. (King Penguin), 1951. 4*s*. [Illustrated by 16 coloured plates ; topical in 1953].

JOHN SPEED'S ATLAS OF ENGLAND AND WALES. 40 plates from John Speed's Pocket Atlas of 1627, introduced and described by E. G. R. TAYLOR. (King Penguin), 1951. 3*s* 6*d*.

TOPOGRAPHY

INNAN SVERIGE BLEV SVERIGE, by CARL AXEL MOBERG. Hugo Gebers Förlag, Stockholm, 1951. Kr. 7.50. [A concise account of Sweden's prehistoric and early historic archaeology, well illustrated, with useful chronological table].

LA HOUGUE BIE, JERSEY : published by the Société Jersiaise (the Museum, Pier road, Jersey), 1951. [This new edition of the original brochure owes most of its excellence to the plans and editorship of Major N. V. L. RYBOT, which are a model of their kind. La Hougue Bie is a famous megalithic burial-chamber in a great mound, excavated in 1924. Probable date, Early Bronze Age].

SCIENTIFIC SURVEY OF SOUTH-EASTERN SCOTLAND: British Association, 1951. [Ten pages and three original distribution-maps by Prof. Stuart Piggott: 3 pages and map of Roman period by Kenneth Steer; other articles on geography, climate, medieval times, growth of Edinburgh, etc. Prepared for Edinburgh meeting, but is it still obtainable, and if so where and how much? A very useful up-to-date handbook].

ARCHAEOLOGY AND NATURAL RESOURCES OF NATAL: published for the University of Natal by G. Cumberlege, 1951. 30*s*. [The archaeology, 29 pages, entirely palaeolithic, by the professor of Classics; the rest covers geology (emphasizing culpable lack of government support), climate, resources: fully and well illustrated and a most commendable effort].

BACKGROUND FOR BEAUTY, by ARNOLD SILCOCK. Andrew Melrose, London, 1951. £2 10*s*. [Its range covers the whole of the Old World from cave-paintings down—or is it up?—to Picasso: some lovely (and unlovely) pictures sumptuously produced, floating in a welter of verbiage. Maiden Castle's eastern defences appear strangely disguised as neolithic art; how the makers would have laughed!].

THE ORIGINS OF EUROPEAN THOUGHT about the Body, the Mind, the Soul, the World, Time and Fate; new interpretations of Greek, Roman and kindred evidence; also of some basic Jewish and Christian beliefs, by R. B. ONIANS. Cambridge, 1951. £2 5*s*. [A learned and scholarly, long-matured product; Professor Onians digs deep into the meaning of words to the thought and practice behind it. It is a fascinating if rather bewildering quest].

THE MINOAN-MYCENAEAN RELIGION and its survival in Greek religion, by MARTIN P. NILSSON (Lund University); 2nd, revised, edn., 208 figs in text: Gleerup, Lund, 1950. Kr. 50. [Now the standard work on the subject. The 1st edn. was reviewed by Prof. Wace in 1927 in the 1st vol. of ANTIQUITY, pp. 370-2].

PRELUDE TO HISTORY; a study of Human Origins and Palaeolithic Savagery, by ADRIAN COATES, with a foreword by GLYN E. DANIEL. Methuen, 1951. £1 2*s* 6*d*. [A useful general handbook of the palaeolithic period, with chapters on History and the Prehistorical, Primitive Mind, Primitive Society, the passing of the Hunter Economy. A book eminently suitable for the general reader and beginner].

UNTERSUCHUNGEN AN DEN CARACALLATHERMEN, von ERIKA BRODNER: de Gruyter, Berlin, 1951. DM. 25. [A useful and scholarly study of the Baths of Caracalla at Rome, illustrated by many plans, elevations and photographs not only of Caracalla's but of many others in Italy, N. Africa and Ephesus].

THE COMPARATIVE ARCHAEOLOGY OF EARLY MESOPOTAMIA, by ANN LOUISE PERKINS. Or. Inst. of Chicago, Studies in Or. Civn., No. 25: Univ. of Chic. Press, 1949. $8.00. [We apologize for our previous failure to notice this very useful compilation; it is one of those books that make the writing of other books possible—and in fact this one has already done so. Indispensable to all students of the Near East, but should now be supplemented by Delougaz and Gawra II].

THE EGYPTIAN COFFIN TEXTS: IV. Texts of Spells 268–354, by ADRIAN DE BUCK (Leyden): Or. Inst. Chic. publns., Vol. 67, 1951. £3 15*s*. [Transcriptions of the original inscriptions, without translations, which presumably will eventually be published also; this volume is for specialists only].

BOOKS RECEIVED

The Excavation of Medinet Habu, Vol. iv: the Mortuary Temple of Ramses iii, Part ii, by Uvo Holscher, with contributions by Rudolf Anthes, translated by Elizabeth B. Hauser. Or. Inst. Chic. publns., Vol. 55, 1951. [A splendid concise but adequate account, with plans, restorations, photographs and coloured drawings, of the temple (12th cent. b.c.), built as a fortress but rather for display than use].

Atlas of Islamic History, compiled by H. W. Hazard: Princeton Univ. Press (London: G. Cumberlege), Vol. 12 of Princeton Oriental Studies, 1951. £1 5*s*. [A useful compilation showing the Islamic World century by century down to the present, with a very handy conversion-table of dates. Minor criticisms of detail:—The Shilluk (marked 7th-16th cents.) generally supposed only to have arrived there in 16th; Islamic penetration of S. Ethiopia in 12th cent. (Sultanate of Shoa, inscribed tombstones in Rift Valley) not indicated].

Swedish Archaeological Bibliography, 1939–48, ed. by S. Janson and O. Vessberg: publd. for the Swedish Arch. Soc. by Almqvist and Wicksell, Uppsala, 1951. Kr. 25. [This admirable undertaking, in English, to be repeated at five-yearly intervals, covers not only Sweden but also the work of Swedish archaeologists all over the world. Their work is fully summarised (pp. 9–291); then comes the bibliography proper (pp. 292–360) of 1244 items].

Paleontology and Modern Biology, by D. M. S. Watson. Yale University Press. $4.00. (London: G. Cumberlege, £1 5*s*), 1951. [A fascinating anatomical study, but outside the scope of Antiquity].

The Story of Pueblo Pottery, by H. M. Wormington. Denver Museum of Natural History, Museum Pictorial No. 2. $1.50 to subscribers. [Traces the development of this still living craft for more than 1500 years, and illustrates it by five photographs by Arminta Neal. The style, aesthetically satisfying, recalls that of the early prehistoric painted pots of Iraq and Susa].

The Stone Age Cultures of Northern Rhodesia, by J. D. Clark: published by the S. African Archaeological Society, P.O. Box 31, Claremont, Cape, 1951. £1 1*s*. [This is a book that was very badly needed, bringing together what is known about palaeolithic and neolithic man and the cultural and climatic succession in the Upper Zambezi valley and its tributaries. Authoritative and indispensable for all prehistorians everywhere].

Summary Guide to the Department of Antiquities, Ashmolean Museum, Oxford: printed for the Visitors, 1951: with illustrations 5*s*, without, 2*s*. [A useful account, with a brief history of this famous old institution of 17th cent. origin: 86 pages and 73 plates, well worth the extra 3*s*, but one has the impression, perhaps erroneously, that not all the objects illustrated are on exhibition].

Gelande und Karte, by Eduard Imhof: Eugen Rentsch, Erlenbach-Zürich, 1950 [A finely produced manual of topography and cartography, profusely illustrated by plans, diagrams and illustrations].

Lost City of the Incas: the story of Machu Picchu and its builders, with 59 plates and a map, by Hiram Bingham: Phoenix House, London, 1951. £1 1*s*. [A popular account of this city and its excavation].

English Drawings of the 10th and 11th centuries, by F. Wormald. Faber and Faber, 1952. £1 10*s*.

THE ARCHAEOLOGICAL JOURNAL, Vol. CVI, Supplement. London, Royal Archaeological Institute of Great Britain and Ireland, 1952. 12*s* 6*d* to non-members. [This Memorial Volume to Sir Alfred Clapham contains a memoir by C. A. Ralegh Radford, a bibliography by Vera M. Dallas, four posthumous articles by Clapham, and papers on Earthworks by R. E. M. Wheeler, on Bishop Alnwick of Lincoln by A. Hamilton Thompson, and on the West Front of Peterborough by Geoffrey F. Webb.

EARLY ENGLISH MANUSCRIPTS IN FACSIMILE. Vol. I, THE THORKELIN TRANSCRIPTS OF BEOWULF, edited by Kemp Malone. Copenhagen: Rosenkilde and Bagger, 1951. Limited to 50 copies for Great Britain. 15 *guineas net*. [The two transcripts here reproduced in collotype facsimile were made for the Icelandic scholar G. J. Thorkelin in 1787, before the edges of the Cottonian manuscript, badly scorched in the fire of 1731, had begun to crumble away. They are therefore of the highest value for the textual study of Beowulf]. H.P.R.F.

THE ABBEY AND BISHOPRIC OF ELY, by EDWARD MILLER. Cambridge University Press. 25*s net*. 1951. [This first volume in the new series of Cambridge Studies in Medieval Life and Thought is sub-titled 'the Social History of an Ecclesiastical Estate from the 10th century to the early 14th century'. Mr Miller discusses the origins of the Ely estate, its organization before and after the Norman Conquest, the peasantry, feudal relations, jurisdiction, and administration. A learned and weighty contribution to our knowledge of the period]. H.P.R.F.

ANGLO-SAXON WRITS, by F. E. HARMER. Manchester University Press. 56*s*. *net*. 1952. [After an introduction of nearly 120 pages, dealing exhaustively with the evolution of this branch of diplomatics, Miss Harmer gives the text of all the extant Old English writs, with a translation and a full commentary. Her work is on the highest plane of scholarship. The volume will surely rank with those of Liebermann, Attenborough, Whitelock, and Robertson, as a classic source-book for the Old English period]. H.P.R.F.

Notes and News

THE ANTIQUITY OF BANGOR, CARNARVONSHIRE*

The 14th centenary of the founding of the see of Bangor was commemorated on June 12 last, in Bangor Cathedral. The celebration asserts the claim of the diocese and of its cathedral site to be the most ancient in Great Britain with an uninterrupted history; for Bangor has been the bishop's seat in Gwynedd, western North Wales, since the reign of King Maelgwn (Gildas's Maglocunun), who died in 547.

All the dioceses of Roman Britain were engulfed in the Anglo-Saxon invasions. St. Ninian's see in Galloway finally vanished in the 8th century. The earliest see in Wales, that of St. Dubricius, which presumably included the whole of Wales, became three dioceses when he retired in 546. Its successors in Mid and South Wales either disappeared during the Celtic period, like Llanbadarn, or were transferred elsewhere, as St. Teilo's diocese was moved to Llandaff within the next 30 years. St. David's was founded later. The distinction of Bangor is that it claims to have succeeded to the portion of St. Dubricius's jurisdiction situated in North Wales and to have retained its status unaltered since 546.

Bangor was founded by St. Deiniol, the immediate successor in North Wales of St. Dubricius. It is from St. Dubricius that Wales derives its traditions in an unbroken line. As the kings of Gwynedd from 547 to 1282 ruled by virtue of their descent from King Maelgwn, so have the bishops of Gwynedd in Bangor exercised their episcopal rights by succession from St. Deiniol, Maelgwn's bishop. Here the past still casts its mantle over the present. Just as the diocese still bears the name of Bangor, so the rural deaneries are still called by the old territorial names, some of them older even than 'Bangor'. The old Celtic abbeys still serve as parish churches. The Welsh language still prevails and is in regular use in the churches, not only for the customary services but also for ordinations, confirmations, and other episcopal occasions, serving to emphasize the continuity of life that links the years 546 and 1952.

St. Deiniol was the son of Dunawd, whose father, Pabo, belonged to a North British reigning family that some generations later provided three kings celebrated as opponents of the earliest Bernician rulers. Pabo's name and generation place him among the allies who accompanied Cunedda, Maelgwn's great-grandfather, on his expedition into Wales about 460. That expedition ended in the expulsion of the Irish immigrants from all the regions between Rhyl and Cardigan, and as his share of the spoils of conquest Pabo received lands near St. Asaph and around Bangor and in Anglesey.

The Bangor founded by St. Deiniol was the earliest Bangor, the 'Bennchor Moer in Britannia' of the Irish chronicles. From it St. Comgall's Bangor in Ireland and the Flintshire Bangor both took their name, which signifies the wattled enclosure surrounding the monastery precincts. It was, however, overshadowed by the greater fame of the Flintshire Bangor, Bede's Bancornaburg, the Bangor of the martyred monks of the Battle of Chester; and this, together with the erroneous identification of Bede's Dinoot of 603 with Dunawd ap Pabo, St. Deiniol's father, created the belief that this Bangor was the earlier. The truth is that the Carnarvonshire Bangor was first founded as a monastery as early as about 520, the Irish Bangor about 558, and the Flintshire Bangor not before 547.

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* Reprinted from *The Times*, 31 May, p. 7, by permission of the Editor.

The principal difficulty in reconstructing Celtic church history in Wales is the lack of an exact chronology, such as Bede supplies for England. All that remains is the comparative evidence of epigraphy and dedications. Inscribed stones of the 5th and early 6th centuries are much more numerous west of the Conway than in Flintshire; Penmachno near Bettws-y-Coed possesses a Chi-Rho inscription and another stone bearing the name of Justin, Roman Consul for 540. The great Gwynedd church foundations of Towyn (Merioneth), its daughter monastery of Bardsey, and Holyhead and Penmon in Anglesey are all of Maelgwn's reign. St. Deiniol belonged to the same generation as the king, and Bangor served that part of Gwynedd not touched by the other great foundations, the heart of the kingdom.

The year 547 brought the Yellow Plague which carried off King Maelgwn and innumerable other victims. A flight to Brittany left South Wales almost deserted; but North Wales received an influx of refugees, vanquished in a civil war in North Britain, where also the Angles were now establishing themselves in Bernicia. The consequent alliance between Wales and these Northern Britons transferred the strategic centre of North Wales to Flintshire. The refugees, under the King of Rheged, St. Deiniol's cousin, acquired temporary homes in the Vale of Clwyd, where their bishop, St. Kentigern of Glasgow, founded his church at St. Asaph. New churches appeared in these regions bearing the names of the sons of the saints of Carnarvonshire and Anglesey—a sure indication of an eastward migration, which probably included St. Deiniol himself, to judge from his churches at both ends of Bala Lake and at Bangor-is-coed in Flintshire.

No details of St. Deiniol's life have been recorded, but from the pre-eminence of his two monasteries he must have been one of the young nobles who followed King Maelgwn to the monastic school of St. Illtud—Gildas's '*paene totius Britanniae magistrum elegantem*'—where he remained after the king's departure. Who ordained and consecrated him is likewise unknown, although a credible tradition attributes his consecration to St. Dubricius, when this saint was succeeded by three bishops simultaneously—by St. Deiniol in North Wales, St. Padarn in Mid Wales, and St. Teilo in South Wales. The *Annales Cambriae* states that St. Deiniol died in 584. But the dates given by that chronicle for the deaths of St. David and St. Kentigern are twelve years too late, and that for St. Deiniol's death may be equally inaccurate. If so, the correct date would be 572, and this would accord much better with the deaths of his contemporaries, Gildas and St. Cadoc.

It is exceedingly difficult to envisage a diocese in a Celtic church, a church with Gallican usages and ministering to a tribal pattern of society. The only parallel account is Bede's description of the Celtic mission in Northumbria and Mercia before Archbishop Theodore of Tarsus reorganized the Church of England on Roman lines in 672. Here diocese and kingdom were identical; there was no great mother church radiating influence and authority throughout the diocese; there was no archdeacon or diocesan executive. The secret of the bishop's authority lay in his reputation for holiness rather than in any official prestige. He had his episcopal status, but his personal worth counted for far more than any pride of place. Such, too, were the conditions in Wales, where a bishop's diocesan functions were principally those of ordaining the clergy and consecrating the holy oil for confirmation, which was, however, administered by the priests.

In 546 almost all Britain west of a line from Bournemouth to York was still unoccupied by the English, and we can infer from Gildas and the *Life of St. Samson* of Dol in Brittany that the ancient British church was still functioning adequately in the western half of what is now England. The first bishop known in Wales, St. Dubricius, is recorded about 500 near Ross-on-Wye. Whence came the episcopal dignity to that remote region ? Analogy with the flight of the Archbishops of York to the fastnesses of the West Riding from the Danes suggests that the clergy of the Roman city of Gloucester must have similarly fled to Ariconium, 15 miles to the west. These clergy did not return, for the work developed westwards.

St. Dubricius is thus the connecting link between the ancient British church and the Celtic church in Wales. In his day the two churches were one in all their customs, usages, and traditions, and the identity remained even after the English had pressed westwards to Offa's Dyke and the authority of Canterbury had gathered the entire English nation into the Roman obedience. The church of the Welsh people was still the ancient British church and followed the ancient paths.

PUBLICATION OF OFFICIAL EXCAVATIONS

The policy of the Ministry of Works (Ancient Monuments Inspectorate) is to publish in established archaeological journals, national, county, etc., because for a century or more they have been the acknowledged means of publishing reports of excavations. It is in their pages that most archaeologists expect to find such reports, rather than in book-form, official or otherwise.

Reports on the following official excavations have been published during the past 13 years. Those published during the war contain no reference to the Ministry of Works or even to the fact that the excavation was officially sponsored and conducted. This was for reasons of security. Those published since the war normally mention the Ministry, but not very prominently, because the Ministry does not make a habit of blowing its own trumpet. It has sometimes been stated in the report that part of the cost of production has been borne by the Ministry.

I. NEOLITHIC AND BRONZE AGE

1. ENGLAND

BERKSHIRE. Barrow Hills Field, Radley. *Oxoniensia*, XIII, 1–17.
CAMBRIDGESHIRE. Snailwell Barrows. *Proc. Cambs. Ant. Soc.*, XLIII, 30–49.
DORSET. Crichel Down Barrows. *Archaeologia*, XC, 47–80.
HAMPSHIRE. Beaulieu Heath Barrows. *Proc. Prehist. Soc.*, IX, 1–27.
  Hurn Barrows. *Proc. Hants. F.C.*, XV, 248–62.
  Stoney Cross Barrows. *Proc. Prehist. Soc.*, IX, 1–27 (part).
OXFORDSHIRE. Dorchester Henge Monuments. *Publ. Ashmolean Museum*, 1951
  Stanton Harcourt Circle and Barrows. *Oxoniensia*, VIII–IX, 19–63 and X, 16–41.
SOMERSET. Charmy Down Barrows. *Ant. J.*, XXX, 34–46.
  North Stoke Barrow. *Ant. J.*, XXX, 34–46.
  Ston Easton Barrows. *Proc. Som. Arch. Soc.*, XCIII, 39–67.
YORKSHIRE. Quernhow Barrow. *Ant. J.*, XXXI, 1–24.

2. WALES AND MONMOUTHSHIRE

BRECONSHIRE. Sennybridge Stone Circle and Barrow. *Arch. Camb.*, 1943, 169–94.
GLAMORGAN. Simondston and Pond Cairns, Bridgend. *Archaeologia*, LXXXVII, 129–80.
  Fairwood Common Barrow. *Arch. Camb.*, 1944, 52–63.
  Llandow Barrows. *Archaeologia*, LXXXIX, 89–126; *Ant. J.*, XXI, 97–127; ANTIQUITY, XV, 1941, 142–61.

MONMOUTHSHIRE. Crick Barrow. *Arch. Camb.*, 1940, 169–91.
PEMBROKESHIRE. Letterston Barrows. *Arch. Camb.*, 1948, 67–87.
Talbenny Barrow. *Arch. Journ.*, XCIX, 1–32.

3. SCOTLAND

AYRSHIRE. Monkton Urnfield. *Proc. Soc. Ant. Scot.*, LXXVIII, 131–4.

II. EARLY IRON AGE

1. ENGLAND

DORSET. Marnhull Settlement. *Proc. Dorset Arch. and N.H. Soc.*, LXXII, 20–75.
HERTFORDSHIRE. Park St. Belgic huts. *Arch. Journ.*, CII, 21–110 (part).
LEICESTERSHIRE. Bredon-on-the-Hill Camp. *Trans. Leics. Arch. Soc.*, XXVI, 17–82.
OXFORDSHIRE. Langford Downs Settlement. *Oxoniensia*, XI–XII, 44–64.
Linch Hill Settlement. *Oxoniensia*, VIII–IX, 19–63 (part).
WILTSHIRE. Boscombe Down West Settlement. *Wilts. Arch. Mag.*, LIV, 123–68.

2. WALES AND MONMOUTHSHIRE

CARDIGANSHIRE. Henllan Camp. *Arch. Camb.*, 1945, 226–40

III. ROMAN

1. ENGLAND

CUMBERLAND. Cardurnock Roman Fort. *C. and W. Trans.*, XLVII, 78–127.
DORSET. Marnhull Roman Settlement. *Proc. Dorset Arch. and N.H. Soc.*, LXXII, 20–75 (part).
HAMPSHIRE. Stoney Cross Roman Road. *Proc. Prehist. Soc.*, IX, 3.
HERTFORDSHIRE. Park St. Roman Villa. *Arch. Journ.*, CII, 21–110.
OXFORDSHIRE. Akeman St., Crawley. *Oxoniensia*, VII, 109–11.

2. SCOTLAND

CAITHNESS. Skitten Broch, Wick. *Proc. Soc. Ant. Scot.*, LXXXII, 124–45.

IV. DARK AGES

1. ENGLAND

BERKSHIRE. Greenham Common Linear Earthwork. *Arch. Journ.*, C, 177–86.
HAMPSHIRE. Beaulieu Heath Linear Earthwork. *Proc. Hants. F.C.*, XVI, 166–7.
Stoney Cross Linear Earthwork. *Proc. Hants. F.C.*, XVI, 166–7.

2. WALES AND MONMOUTHSHIRE

GLAMORGAN. Hirwaun Mounds. *Arch. Camb.*, 1942, 77–92.

V. MEDIEVAL AND LATER

1. ENGLAND

HERTFORDSHIRE. Nuthampstead Moat. *Ant. J.*, XXVI, 138–44.
KENT. Nash Court Mounds, Boughton. *Arch. Camb.*, LXIV, 35–8.

NOTES AND NEWS

TERMINOLOGY IN EGYPTIAN PREHISTORY

We publish the following note by Professor Gordon Childe being in full agreement with the contents. Editor.

A commonly recognized terminology is obviously a precondition for advance in any science. In prehistoric archaeology it can indeed hardly be claimed that a rigorously scientific set of terms has been agreed upon, but in most of its branches sufficient agreement has been reached for authors and students to be able at least to understand one another. Even this measure of agreement seems not yet to have been finally achieved in Egyptian prehistory. Its foundations were laid by the Frenchman de Morgan and the Englishman Flinders Petrie, who first discovered the predynastic background of the Pharaonic monarchy, but it was Petrie and his school who really developed its study and published the decisive material—in English. At first the new material, derived for the most part from cemeteries in Upper Egypt, was termed simply Predynastic and subdivided into Early, Middle and Late.[1] By 1917 indeed it was apparent that the several assemblages that successively dominated Upper Egypt in the prehistoric period were more than mere stages in the development of a single culture,[2] but it was still practicable to speak of an Early or a Middle Predynastic culture, using the term in the dual sense of ‘ an assemblage of relics repeatedly associated together ’ and of the period of time during which that assemblage characterized the record in Upper Egypt. Some authors, however, preferred to emphasize the contrasts rather than the continuity by speaking of First and Second (Predynastic) cultures.

With the discovery in 1926 of the Badarian culture, that was obviously earlier than ‘ Early Predynastic ’, both systems became obsolete. Early Predynastic was no longer ‘ Early ’ nor ‘ First ’. Subsequently Austrian and Egyptian excavators brought to light at Merimde and Maadi in Lower Egypt prehistoric cultures that did not fit into the Upper Egyptian sequence. In this situation Petrie proposed a revision of terminology in accordance with the well established archaeological practice. He replaced the terms Early, Middle and Late Predynastic by appellations taken from the names of sites where the relevant assemblages were particularly well or alone represented, as at Tasa, Badari el Amrah, Gerzeh and Semaineh. Accordingly, Tasian, Badarian, Amratian, Gerzean and Semainian represented both recurrent assemblages of archaeological traits and the periods of time defined by their currency in Upper Egypt. The newly found cultures of Lower Egypt could then be given independent names, likewise derived from type sites—Merimdian and Maadian. But, the archaeological record being most complete in Upper Egypt, it was still useful to ask whether Maadi was Gerzean or Semainian in age, i.e., fell into that portion of the continuous Upper Egyptian sequence defined by a Gerzean or a Semainian assemblage.

As a result, the names Badarian, Amratian, Gerzean and Semainian have been generally accepted not only in Great Britain, but also in America, Austria[3], Egypt[4], etc. In the light of Miss Cantor's article, ‘ Gerzean or Semainian ? ’[5] the last of Petrie's terms, should perhaps be dropped, but that will not impair the system, which is at once scientific, in strict harmony with archaeological practice as reaffirmed by the designations formally adopted for Mesopotamian prehistory by the Baghdad Conference in 1930, and, unlike the latter, euphonious. Only in Germany, where Egyptologists only late turned their attention to prehistoric remains, did they find it necessary in the interests of 'Germanic science' to adopt another system. In his *Grundzüge der ägyptischen Vorgeschichte*, published in 1927, Scharff still spoke of the *Erste* and *Zweite Kultur*. With the recognition of the Badarian, earlier than the '*Erste*,' and of the Merimdian, neither *Erste* nor *Zweite*, such terms became as meaningless as Early or Middle Predynastic. Scharff took a peculiarly illogical and characteristically German way out. Amratian and Gerzean became Nakada I and Nakada II, and were thus contrasted with Badarian and Merimdian.

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[1] So, e.g., Peet, *CAH.*, 1 (1924), 248.

[2] Petrie, *Prehistoric Egypt*, 1917.

[3] e.g., Menghin, *Weltgeschichte der Steinzeit*, Vienna, 1931.

[4] e.g., Amer and Menghin, *Excavations of the Egyptian University in the Neolithic Site at Maadi.* First Preliminary Report. (Season 1930–31). Faculty of Arts Publication No. 19. Cairo. 1932 ; and Huzayyin, *The Place of Egypt in Prehistory. A correlated study of climates and cultures in the Old World.* Cairo. 1941. Mémoires présentés à l'Institut d'Égypte. Tome 43.

[5] Kantor, ‘ The Final Phase of Predynastic Culture : Gerzean or Semainean (*?*)’, *JNES.*, 3, 110.

Now doubtless both Amratian and Gerzean assemblages had first been recognized in the Naqada cemeteries and were well represented there. But surely this fact is itself a disqualification for a culture name, since it has never been archaeological practice to give contrasted assemblages names that have to be distinguished by numerals—numerals should be used to distinguish phases in the development of one and the same culture. Moreover, at Naqada the Gerzean culture is not found pure but blended with obvious survivals of the preceding Amratian. Scharff's terminology has nothing to recommend it, save its potential use as an instrument for the spread of Germanic culture.

But the next stage in that spread, the Nazi revolution, enriched Egyptology and other branches of learning in America and Britain with numerous refugees from Nazi tyranny. As mature scholars, such were naturally prejudiced in favour of the terminologies, however illogical, in which they had been brought up. But surely they should not attempt to impose these terms upon luckless English-speakers who have been more fortunate in their educational backgrounds.

I am not an Egyptologist and have no qualifications to speak of the internal merits of any system of names. But as a prehistorian interested in the development of human culture as a whole, I have legitimate grounds for asking that Egyptological literature published in English shall be written in terms which I and, what is much more important, my students can understand, and that students should not be unnecessarily distracted and confused by an illogical, unscientific, foreign terminology. One book thus germanised has already been published with a substantial subsidy from a British university, and more are expected. Much though we desire the collaboration of German colleagues, we must surely demand that their voluminous writings shall not make unintelligible to the rising generation, the English classics of Petrie, Brunton and Caton-Thompson—nor incapable of earning dollars in America. V. GORDON CHILDE.

Reviews

CONCENTRIC CIRCLES AT LLWYN-DU BACH, PENYGROES, CAERNARVONSHIRE. *By* DR GERHARD BERSU *and* W. E. GRIFFITHS. *Archaeologia Cambrensis*, 1949, 173–206.

This excavation was undertaken by the Caernarvonshire Excavation Committee in 1947, under the direction of the authors. Attention was first called to habitation-sites of this kind by Messrs Hemp & Gresham in ANTIQUITY (XVIII, 1944, 183). They consist of a round central wall, the remains of the house, surrounded by two other walls (here called rings) disposed more or less concentrically. At Llwyn-du Bach the central house was approached by a pathway between two flanking walls joining the middle and outermost rings, where stood one, or two, wooden doors and probably also 'some wooden superstructure, such as a gate-tower'. The absence of any adjacent cultivation-plots and the presence of remains of radiating (ranche ?) walls shows that stockbreeding rather than agriculture was the main support of the inhabitants. The outer enclosure was probably for cattle.

The roof of the central house rested on timber supports placed asymmetrically in relation to the wall, but with its main axis aligned with the door; it was not therefore an aisled round house, but to be compared with some Iron Age C huts at Maiden Castle. No dateable finds were made, but the excavators concluded that it was probably built in post-Roman times. Pre-Roman influences could hardly have penetrated so deeply in the short time between their introduction and the coming of the Romans, and there were no Roman imports found, as elsewhere in this period.

The suggestion of a structure over the main entrance to the outer enclosure recalls those at Axum in Ethiopia over the walls round the modern houses and the churches. There is a passage between two short side-walls about 4 to 6 metres long, with a room above; the whole is built of stone. The central buildings are both round and rectangular, the round houses having aisles between the roof-supports and the outer wall, as have also the (round) churches. These buildings are described and planned in Band 3 of the Deutsche Aksum Expedition's report (1913) which is a valuable mine of information about primitive houses.

The report on the excavation of Llwyn-du Bach is a model of its kind and an example of what meticulously careful observation can extract from slight differences in soil, disposition of stones and negative evidence. One's only regret is that the best diggers seem doomed always to happen on the worst sites—and vice versa. By 'worst' is meant sites that deny them even that bare minimum of dateable finds that they have a right to expect, even in the potless Dark Age of a poor mountain land. Here the habitation-level of the house was eroded away by rain-scour in the short interval between its desertion and the growth of a protecting cover of turf, and was only inferred from the presence of its black soil in some holes. This, the existence of several layers in hearths and certain repair-work show that the house was occupied for some length of time.

The Cornish 'rounds' also consist of triple concentric enclosures, whose entrances are marked by a pair of large standing stones in the surrounding walls. The central enclosures are too big to have been single houses; but the plan seems to be here, too, associated with cattle-raising, the space between the middle and outer walls being used for the animals. In Cornwall also are remains of long walls that can only be ranche-boundaries, the equivalent of these earthen bivallates that cover the chalk downs of Wessex and radiate from such habitation-centres as Sidbury and Quarley. This concentric system is, however, very widely distributed both in time and space, and is a natural adaptation of a pastoral people. The subject is touched on briefly in the article on Cornish walls (ANTIQUITY X, 1936, 162–74).

Mr Ralegh Radford contributes a most interesting note on the relation between the traditional historical accounts of early Welsh society and the archaeological evidence of this and other sites, which confirms them. O.G.S.C.

THE TECHNOLOGY OF A STONE AGE PEOPLE IN NEW GUINEA. *By* BEATRICE BLACKWOOD. Occasional Papers on Technology, 3. *The Pitt Rivers Museum, Oxford,* 1950. *pp.* 60, 19 *text figures,* 16 *plates. Price* 10*s* 6*d.*

STONE AGE CRAFTSMEN: STONE TOOLS AND CAMPING PLACES OF THE AUSTRALIAN ABORIGINES. *By* S. R. MITCHELL. *Tait Book Company, Melbourne,* 1949. *pp.* 211, 45 *figures,* 16 *plates. Price* £1 7*s* 6*d.*

OURSELVES WRIT STRANGE. *By* ALAN MARSHALL. *George Allen & Unwin, London,* 1950. *pp.* 321, 19 *plates. Price* 15*s.*

To the modern archaeologist trying to expand the scope of his subject from the 19th century ' collector's catalogue ' approach to one of wider human significance, it must sometimes seem that his anthropological colleague lays exasperatingly little emphasis on the industrial and economic bases of primitive society as opposed to such purely social abstractions as kinship systems, marriage customs and the like.

Since few today would care to rely on the material of comparative sociology of living peoples to reconstruct the course of human social evolution, it follows that the only hope of enlightenment is that offered by the fullest possible investigation of the surviving material culture of extinct societies. In this connection it is difficult to over-estimate the value of exact circumstantial observations on the technology of living primitive communities.

Indeed one would have thought that the links between prehistory and ethnography were so self-evident and of such wide significance that few opportunities would have been lost of combining the two approaches to the basic problems of human development.

Yet it is unfortunately a fact that nearly all recent primitive communities became extinct before even the most superficial observations were recorded which could be of real help to the prehistorian. What the prehistorian requires is not merely a vague statement that the inhabitants of such and such an area live mainly by hunting, or make use of the bow, but rather a precise account of the number of animals killed and in what manner, the tools employed, and the methods used in their manufacture. Quantitative data are particularly useful, and so are clear explanatory diagrams, and photographs of the processes in actual operation.

In all these respects the first work named above maintains a remarkably high standard and provides a most enlightening account of the industrial activities of a community that may be described as on the borderline between ' mesolithic ' and ' neolithic '.

While it does not of course follow that the same objects and processes were not turned to quite other uses and evolved in quite different ways in other areas, it can scarcely be denied that such a record is worth a hundred hypothetical material cultures evolved in the mind of a civilised prehistorian sitting in his armchair. Here at least is *one* way in which artifacts of the kind were *certainly* made and used. Particularly fascinating are the photographs showing every stage in the shaping, grinding, and hafting of a stone adze, all the tools used being of wood, stone or bone. Another interesting passage deals with arrows. While their general form is strictly governed by function and custom, their size and decoration are very largely a matter merely of individual preference.

As a measure of the urgency of such investigations it may be noted that the technology of this particular community was altered fundamentally by the introduction of European products within two years of the date of the observations.

A further case in point is supplied by the second work under review, which is a careful and painstaking account of industrial remains from surface sites throughout the sub-continent of Australia. As a general survey of such remains it is likely to remain for long the standard work on the subject. The treatment is in the main purely archaeological, that is to say artifacts are classed in accordance with their shape or method of manufacture with few serious deductions as to how they may or may not have been used, and how many distinct tool-ideas they may really represent. The truth is that the observations that might so easily have enabled such deductions a generation ago, can now never be made. Despite the undoubted usefulness of this study to any future archaeologist working in Australia, it is sad to think of the brilliant contribution to stone-age archaeology in general that would have been offered by the systematic study of the stone tools of even a single community, if it had included such details as exact descriptions of the purposes served and the precise variation in form of the tools used for a specific task. But this is not the fault of the author who has been at pains to collect such scattered (and often ambiguous) information as the ethnographical literature affords. On the whole the numerous illustrations are adequate to their purpose, though the data required by any one wishing to reconstruct the full process of manufacture of the different tool types is not always clearly recorded.

The third book is of little direct relevance to prehistory, though it is perhaps the sort of work which it would be good for most prehistorians to read and ponder from time to time. It consists of a set of charming anecdotes told in a very informal style by a layman with no anthropological training who has travelled widely among the modern descendants of the aborigines in Northern Australia. The point which emerges most clearly is the essential humanity of these people, and the ease with which they can be approached if ordinary good manners are observed and a little common sense with regard to their particular customs. Good and bad, gifted and stupid individuals are to be expected in every society, but it is perhaps more surprising to learn that among the descendants of typical kangaroo and wallaby hunters, after a single generation, are some that can read our Shakespeare with understanding and keen enjoyment.

C. B. M. McBurney.

LE GISEMENT PALÉOLITHIQUE DE SIDI ZIN. *By* E. G. Gobert, with a section on fauna *by* R. Vaufrey. Volume 1 of *Karthago*, Revue d'archéologie africaine, 1950. *Printed by Librairie Klincksieck*, 11 *rue de Lille, Paris* vii. *pp.* 51, 17 *figures*, 6 *plates*, quarto. *Price* not stated.

In this handsomely produced short monograph, Dr Gobert, well known to prehistorians as one of the leading authorities on the stone age of North Africa, provides an admirable account of the first important Lower Palaeolithic find from Algeria or Tunisia for over half a century. The site was discovered by Etienne Dumont in 1942 in the course of a geological survey, and yielded to Dr Gobert a fine series of palaeolithic tools *in situ* in four layers in calcareous tufa, and alluvium with an associated vertebrate fauna. The specimens are illustrated in a series of drawings from the incomparable pen of

E. Evrard, and are clearly described by the author in his usual systematic fashion with tables giving the frequencies of the various types in each layer. The photographs of the site are less informative, however, and not well reproduced, though in any case the geological evidence fails to afford any real clue as to the date in the local succession, or the climate, at any rate until further work has been done in the area.

The industries from the three lowest layers are all characterised by large numbers of superbly finished oval and lanceolate hand axes, in hard limestone, together with a smaller number of smaller and rougher flake tools such as points and side-scrapers in flint and quartzite. These are described by Dr Gobert as 'Micoquian' and compared to the similarly classified industries from Mount Carmel. To the author of this review the propriety of using the ill-dated site in the Dordogne as the representative of a widespread stage throughout the Mediterranean basin, has long seemed doubtful, and there would appear to be much in favour of describing both the Sidi Zin and the Mount Carmel series merely as Late or Evolved Acheulean. The main point in both cases is simply the occurrence of highly finished hand-axes with a flake industry from which true Levalloisian or Mousteroid flaking techniques are virtually absent. It may be recalled that the same is not true of Kharga where a gradual transformation of Acheulean into Levalloisian can be detected.

The final series at Sidi Zin consists merely of a relatively small series of flake tools of the same types as the preceding layers.

The fauna is of interest as being the first extensive collection from Northern Africa to be satisfactorily associated with hand-axes, since neither of the well-known collections from Lake Karar or Palikao can be regarded as above criticism in this respect. It is interesting that the assemblage is on the whole of a rather markedly African aspect including the South African variety *Equus Kuhni* and *Connochoetes cf. taurinus*—the blue Gnu of eastern and central Africa. The same observation was made many years ago by the late Professor Boule in his study of the Lake Karar collection, though at Sidi Zin it may be noted are also some traces of the Eurasiatic species *Bos Primigenius* destined to become a dominant form in the subsequent Middle Palaeolithic phase.

C. B. M. McBurney.

ART OF THE NORTH-WEST COAST INDIANS. *By* R. B. Inverarity. *University of California Press*, 1951. (Agents—Cambridge University Press). *pp.* 243, 279 *plates* (5 *in colour*), 1 *map*. £3 15*s* 0*d*.

That portion of the north-west coast of America corresponding roughly to the western seaboard of present-day Canada offers a relatively uniform and highly characteristic environment for human occupation. For a distance of nearly a thousand miles the shore presents a complicated pattern of deep inlets and innumerable off-lying islands. Throughout this zone the climate is temperate and moist giving rise to intense development of coniferous forests and plant life generally. Both are exceptionally suitable for food, clothing and many kinds of industrial activity, while the sea provides animal food in great variety and abundance.

Little documentary evidence is available concerning this area before the 18th century when it was the scene of a flourishing culture of hunting and gathering communities whose social, intellectual, and artistic attainments far surpass those of any other society living by these means known to anthropology.

The only domestic animal was the dog, and cultivation of a few herbs of very minor economic importance the only form of agriculture. The art of food preservation on the other hand was carried to a high degree of development, and certainly must have been an important contributing factor in rendering possible the other forms of cultural activity.

As with most of the more advanced new world cultures the plastic arts played an outstanding part in the life of the people. Elaborate decoration was lavished on houses, graves, boats, clothes and a wide variety of other objects of every-day use, giving rise to a highly characteristic style which seems to have been fully evolved at the time of the earliest European visitors to the area, and to have survived with little change up to the virtual extinction of the culture at the beginning of this century.

The object of the book under review has been to bring together a corpus of outstanding examples of this art, mainly in American public and private collections and hitherto unpublished. From the 260 odd specimens shown, a fair impression of the range and applications of this art may be gathered. Apart from a few pieces showing evident incorporation of European motifs, few if any can be dated on stylistic grounds, though probably none of the examples shown are older than about 200 years. Practically nothing is yet known from archaeological sources of the origins of the tradition. Broadly speaking, though highly individual, it is not without suggestive analogies with other New World styles practised far to the South in Central and Southern America, and the even more remote traditions of Oceania and even New Zealand; it is an art of complicated designs, generally with a well defined symbolical meaning, and often highly decorative in effect, though far removed from natural appearances, and displaying practically no interest in representational or sensual appeal. In all these respects it is closely parallel say to the art of the ancient Maya, though representing an appreciably higher level of achievement than the art of the Aztec or the Tiahuanaco culture of the Andes. Again for the same reasons it is in striking contrast to the art, say, of the last few centuries in Europe, West Africa, or Medieval India, which all show a concentration on natural impressions either through representation or exaggeration wholly foreign to the North-west Indians. Perhaps the nearest Old World analogy is that of the early Chinese bronzes, and it is interesting to recall that some authorities have sought to trace an actual connection between the two.

It must be admitted however that the whole of this fascinating subject is as yet completely shrouded in mystery and a collection like that offered in this book serves merely to whet the curiosity, and to emphasize once again the extraordinary diversity of human taste, and the futility of regarding any one school or type of art as the standard of aesthetic evolution.

The illustrations are on the whole of a fairly high quality; but the style of book production will strike most European readers as unnecessarily florid, and more like that of a department store catalogue than a work of learning. The titles are not very illuminating, though some background on the social side is provided by a few pages of introductory text. A useful bibliography is included and the whereabouts of all the objects are given. C. B. M. McBurney.

HORTFUNDE SÜDOSTEUROPAS [Bronze hoards of S.E. Europe], *von* Friedrich Holste : [50 *plates with museum-notes made by the author, published by the Prehistoric Seminar of Philipp's University, Marburg-Lahn,* 1951. *Price,* DM. 20].

Professor Merhart contributes a Foreword to this posthumous publication of the results of one of the author's tours round the museums of Central and Southern Europe. The multigraphed text consists simply of descriptions of the plates and objects. The plates contain line-drawings of the contents of the various hoards, and are adequate for their purpose. A tabular arrangement, however, giving the plate-reference first, would have facilitated use, for one works from the plate to the text (not *vice versa*) ; and if one is doubtful what the drawing represents—and that is why one usually consults the text—it often takes some time to find the author's opinion. Apart from this defect the publication is excellent, scholarly, and useful. One wishes that more such could be produced for other regions, such as France and Britain. For these hoards are one of the chief sources of our knowledge of the Bronze Age, and they have not yet been made to contribute as much information as they might. It was largely by a study of the contents of hoards that Sir John Evans laid the foundations of the relative chronology of the British Bronze Age. That achievement, based on a single line of evidence was a *tour-de-force*; but the fact that, on the whole, it has stood the test of time so well proves that it was worth doing, and suggests that similar attempts, more broadly based, would give good results. This publication provides some of the material for such.

A casual glance at the drawings suggests possible lines of evolution of some of the objects. A sequence starting with Plate 3 (18), a flat axe with flanges, seems to develop through Plate 3 (17, 19, 13) to a socketed axe, Plate 1 (14, 15) and Plate 10 (6, 5, 4), and perhaps to other types also. Plate 3 (18) looks like the ancestor of the Swiss and French winged axe ; it is found also in Italy, but its home may well be north or east of the Alps. The Iron Age type fibula, Plate 10 (10), seems to be derived from a pin (like Plate 10, 1 and Plate 16, 36) whose flat head became the catch-plate. The hump-backed knives, Plates 2 (2), 9 (12), plainly developed through Plate 48 (40, 20) into the knobbed sickles, Plate 48 (20, 22), which are very common in the hoards here illustrated.

The objects described as pendants, Plate 11 (26) and 18 (7) are almost identical with those hanging from the rim of the wheeled cauldron from Skallerup, Zealand (Childe, Prehistoric Migrations, Oslo, 1950, fig. 163). These two pendants are from Bingula-Divos (Zagreb museum) and from Caransebes (Vršac museum), and both contain other objects that might have come from cauldrons, such as the wheel, Plate 18 (10), and some of the other pendants, one of which, Plate 11 (22) has a bird like the Skallerup cauldron. But of course these bird-chariots are known to extend far into southeastern Europe. We mention these resemblances chiefly because we believe that cauldron-fragments are to be found in hoards more often than has been thought, and when they can be recognised they form a useful chronological and cultural link. Some English hoards contained the ring-handles of cauldrons. O.G.S.C.

THE BIRTH OF CIVILIZATION IN THE NEAR EAST. *By* HENRI FRANKFORT. 116 *pages*, 24 *plates*. *Williams & Norgate*, 1951. 16*s*.

The birth of civilization in the Near East was a unique event as much as the birth of the Moon from the bosom of the Earth. It may be considered ' as the birth of civilization in a general sense '. ' No appeal to foreign influence can explain the emergence of civilized societies in Egypt and Mesopotamia since these lands were the first to rise above a universal level of primitive existence ' (p. 15). Some readers of the Appendix on ' The influence of Mesopotamia on Egypt towards the end of the Fourth Millennium B.C.' may think that ' Egypt ' should be omitted from this sentence.

But the Egyptian and Sumerian civilizations were unique in another sense—in the sense that Henri Frankfort and Gordon Childe, or the Riss and Würm Ice Ages, are unique—and it is to vindicate this uniqueness that the author is principally concerned. His first chapter is entirely occupied with a criticism of Spengler and Toynbee for failing to recognize it. ' Generalizations,' he concludes, ' based on a limited historical experience, and theorizing, however ingeniously conducted, *must* [his italics] fail to disclose the individual character of any one civilization, (p. 31). But is it illegitimate, ignoring the individuality of each, to try and recognize common characters, as Spengler and Toynbee do ? The Commissioners of Inland Revenue, sociologists and anthropologists ignore the individual peculiarities of Frankfort and Childe and treat them as members of an income-group, a class, or a race ; and the utility of their procedure is obvious enough in the first instance, at least to the Chancellor of the Exchequer. I should personally endorse most of the author's specific charges against Spengler and Toynbee, but the general indictment seems based on a misapprehension of their aims ; for the phrase ' civilization in a general sense ', quoted from the first page of Chapter I, betrays an implicit acceptance of the underlying philosophy.

What Dr Frankfort sets out to describe is not the genesis of civilization but the births of two distinct civilizations ; he is at pains to emphasize the individuality of each. Accordingly he gives a catalogue of differences between Egyptian and Sumerian civilizations which could easily be enlarged. Of course the reader must be made to realize what a gulf divides a *pharaoh* from an *ensi* or even a *lugal*, how patent is the contrast between Egyptian and Sumerian art. But a book entitled *The Birth of Civilization*, even if two ' s's ' have been omitted from the title, might condescend to mention another contrast—that with any barbaric or savage art. We can recognize the difference in ideals and conventions inspiring Egyptian and Sumerian—and for that matter Indus, Minoan and Shang—portrait statues ; but we know at once that each artist was trying in his own way to make what we should recognize as a *likeness* of a person. No illiterate or preliterate barbarian ever did or does attempt a likeness recognizable to us.

Within his self-imposed limits Dr Frankfort gives the best simple account of the prehistoric and early historic periods in the valleys of the Tigris–Euphrates and the Nile up to date. He manages to describe the salient features of the prehistoric phases without burdening the reader with technical archaeological details. In Mesopotamia the ugly terms Uruk and Jemdet Nasr are replaced by Early and Late Protoliterate, while in Egypt the familiar and euphonious English terminology (but omitting Semainian) is maintained in spite of German efforts to impose an illogical Naqada I and Naqada II. The account of early Sumerian social organization is firmly based on Diemel and Schneider, with the addition of Jacobsen's somewhat speculative reconstruction of ' primitive democracy ' in a still earlier ' protoliterate ' stage. Well chosen extracts from autobiographies chiselled on tomb walls happily illustrate the ideals of the Egyptian bureaucracy. But though the antecedents are admirably sketched and the childhood is finely portrayed, we seem to miss the birth—what I call the Urban Revolution. Frankfort objects to my term the Urban Revolution, firstly (p. 83) because in Egypt (a) cities ' with the single exception of the capital, were no more than market towns for the countryside '. and (b) ' until the middle of the second millennium B.C., there was no truly permanent capital in Egypt ' ; (c) ' each pharaoh took up residence near the site chosen for his tomb '. Even Memphis, traditionally founded by Menes at the unification of Egypt, was (d) ' originally—and remained for fifteen hundred years—almost exclusively a sacred city ' (p. 81). He omits even to mention Emery's claim to have found at Saqqara, the necropolis of Memphis, tombs of two kings and a queen of the First Dynasty (footnote 1 on p. 81, mentioning only ' high officials of the kings of the First Dynasty ' is somewhat disingenuous). If accepted, as I believe it generally is, it means that Memphis was a capital in the sense of paragraph (c) above for a couple of centuries. In any case all the kings of Dynasties I and II had admitted tombs at Abydos which, according to the same paragraph, would have been a ' capital ' for 250 years. After all Agade and Ur were capitals of Mesopotamia for no longer. But it remains true and significant that the ' county ' (*nome*) was the rallying-point of local patriotism, as in feudal Britain, while in

Mesopotamia, as in the Mediterranean, it was the city. Hence opposition to the pharaohs' authority was led by ' counts ' and not ' Lords Mayor '.

Secondly, though in Mesopotamia cities are admitted, Eshnunna or Kish were not very like Birmingham or Detroit. Most of the citizens, like classical Athenians, or medieval European burghers, farmed land in the city territory. Frankfort goes on to assert that ' every citizen whether priest, merchant, or craftsman, was a practical farmer who worked his allotment to support himself and his dependents ' (p. 63). For, he reminds us, in the Sumerian temple-city, most of those who appear as recipients of wages (rations) from the temple stores—including priests, merchants, and smiths—also held allotments on the temple lands. Now the minimum recorded allotment is 7/8th of an acre ' which would suffice to keep a man '. But we read of a *nubanda* who held 120 acres and another official held some 80 (p. 61). Did the *nubanda* really cultivate 120 acres with his own hands ? Is it not more likely that it was worked for him by some of the small holders ? But, if so, was not the smith's allotment likewise cultivated for him in some like manner so that he actually spent all his time at his craft, but could rely on a guaranteed basic income in addition to his wages ?

Dr Frankfort does not consider this possibility. For him, early Sumerian priests, merchants and craftsmen were part-time specialists and he cites examples from English villages and barbarian tribes of specialist craftsmen who are still primarily farmers. He might have gone on to quote master craftsmen in Tierra del Fuego who specialize in making arrowheads in the intervals of hunting and fishing. All would be equally irrelevant. For Frankfort ignores the useful distinction drawn by C. S. Coon between ' part-time ' and ' full-time specialists '. My ' Urban Revolution ' is just what made full-time specialization possible, i.e., relieved some men of the urgent task of growing or catching their own food that they might devote their full time to that ' intellectual adventure ' of speculative thought which Frankfort has described so brilliantly in *Before Philosophy*, or to the practice of the crafts. In Egypt, Frankfort does not attempt to deny that the scribes, metal-wrokers, weavers and others, described in detail in *The Praise of Writing* and other texts and figured on Old, as well as New, Kingdom tomb walls, were full-time professionals.

Such of course were a small minority in Egypt and Mesopotamia. But they did live on the ' surplus ' food produced by the peasantry and concentrated by the State or the temple. After all Frankfort and I have time to write books and reviews, just because Polish peasants, Canadian farmers and Orcadian crofters produce more eggs and wheat and milk than they consume. That truth was more evident in the childhood of civilization than today. Indeed the extraction and concentration of the surplus was the Urban Revolution.

V. GORDON CHILDE.

TROY, VOL. II : The Third, Fourth and Fifth Settlements, by CARL W. BLEGEN, JOHN L. CASKEY, MARION RAWSON. *Part I*, XXII, 326 *pages* ; *Part II*, 315 *figs.*

Schliemann and Dörpfeld left even less of Troy III–V than of Troy II. And in any case these settlements never seemed very exciting after Schliemann had agreed to identify ' the Burnt City ' with II. (Schaeffer's attempt to restore III to that status is on p. 4 dismissed in a footnote as ' an academic study based on an eclectic scrutiny of all published reports and plans, including our own preliminary accounts, but untrammelled by a corresponding examination of the ruins on the site '. But Volume II has no more to say than Volume I about the massive brick rampart that Schaeffer rediscovered in the literature). Yet their poor remains are described and illustrated as lavishly as those of Troy I and II, and rightly so ; for instance, only the photograph in Fig. 79 shows that a

‘ Face-lid ’ of III is more like the celebrated Vinča specimens than the familiar types. The Cincinnati Expedition has at least shown that Towns III–V were not miserable villages as Dörpfeld had alleged. Nor were they short-lived ; periods of reconstruction can be distinguished within the life of each. But continuity with Troy II was preserved throughout. Even in pottery few new types emerged as ‘ innovations ’. The wheel was of course in use in all towns, but its mass production was surprisingly slow in ousting the old domestic industry ; in IV ‘ the potters seem to have made more general use of the wheel than their predecessors, for hand-made vases are relatively less numerous than heretofore ’, while in V ‘ many pots seem to have been produced by hand but the great majority were now thrown on the wheel ’. Domed ovens came into use in IV but in V we meet a more elaborate version with separate fuel-chamber. On the other hand, in III and IV the proportion of game bones was unusually high, and only in V do cow and pig appear again as the commonest food animals.

Though grey wares are frequently mentioned, these are not represented as precursors of the Minyan, characteristic of VI, nor do the authors indicate any other traits foreshadowing the new culture that dominated that town. On the contrary, ‘ Troy V was overwhelmed in the same movement of peoples that ushered in the Middle Bronze Age of Greece ’. For the date of that event, apart from parallelism with the Cyclades and Helladic Greece, the best clues are provided by numerous close agreements between the pottery of Troy V and that from the level at Gözlu Kale (Tarsus) that yielded imprints of Cappadocian seals. Dr Goldman’s dating as ‘ not later than the 20th century B.C.’ is quoted as given in 1940. After the publication of *Alalakh and Chronology* she would doubtless have written instead ‘ not earlier than the 20th century ’. Everyone will await with excitement the promised volume III, if only to see how Troy VI is to fill even five centuries (1850–1350 ?). V. GORDON CHILDE.

JORDFORDELING OG UDSKIFNING. *By* C. RISE HANSEN *and* AXEL STEENSBERG. *pp.* 485 *with* 16 *folding maps and* 54 *figures in text. Copenhagen,* 1951. *No price stated.*

This study is concerned with the open field villages of Southwest Zealand at the end of the 17th century. As has so often happened, the fiscal needs of the Crown at a point in the past have proved the historian’s blessing. The royal survey of 1681–3 resulted in more than 1800 Register books giving the areas of the strips, the optimum crop, the quality of the soil and the length of the rotation-period through which cropping moved. Meadow and pasture were not surveyed by area but by assessed yield in hay or by capacity to maintain animals. Woodland was assessed by mast, as the English Domesday surveyors assessed land *de x porcis.* Indeed it is with Domesday that the English reader compares this Danish undertaking, with regrets that nothing on such a scale of thoroughness was undertaken by the Stuarts. It is true that we have the Surveys of Crown Lands, the Surveys of some private estates (particularly those of Oxford and Cambridge Colleges) and the terriers of the glebe land. But these comprised only either one holding in a village or one village in a district.

The authors set themselves the task of reconstructing maps from the evidence of the 1681–3 Survey, aided by original maps of the enclosure period *c.* 1770. There are three such maps in the text, which also reproduces the 18th century maps of the same villages for comparison.

The first section of the book (pp. 1–126) describes the source material and the technique of the surveyors. After a short account of the 1770 maps, there follows a comparison of the villages of Hejninge, Grøfte and Snekkerup at this interval of a century. In the first village there were sixteen farms: but a village tradition of an original eight-farm settlement, and a doubling of tenements at some point in the Middle Ages when the village's cultivated area had been expanded. The number sixteen cast its shadow over all the life in the fields. Indeed the connection between the number of tenements and the same number of shares in the open fields is underlined by the fact that a seventeenth holding, the Rectory, a latecomer, had its land outside the common fields in a compact block. The fields were clearly the prize of those who had participated in the original work of clearing and in the subsequent centuries of assarting. There were three Fields, one fallow each year, not exactly equal in area (168, 138 and 120 ha.). There were 35, 41 and 31 bundles of strips (Aas: *anglice* furlong or shot) in these Fields. Many furlongs had 16, or a multiple of 16, strips within them. There was no fixed order within the furlong for any individual's strips, thus suggesting a partition by lot after the first ploughing.

But the the twin halves of the original eight holdings still stood side by side within the furlong, being an original strip now divided into two. As in England, the area of furlongs and the size of strips varied. But between 75 per cent and 85 per cent of the strips had breadths between 9.5 and 19 metres and nearly 40 per cent between 12.5 and 16 metres. Of 1516 strips, more than half lay between 1000 and 3000 square metres.

In 1786 the village was still open, but an organisational change had taken place. *Egalisering*—equalisation—took place *c.* 1720 when the holdings of the farms were redistributed to iron out inequalities which had arisen between holdings despite the conceptual equality. Rents became equal and the tax assessments became equal. Another 13 per cent of newly cultivated land had been added to the arable and this made it possible to increase the size of the furlongs. The authors comment that this would indicate a loose marking of the old furlongs in the Fields and easily ploughed-out ridging in the strips. (Incidentally, when the surveyors of 1681 were searching for former arable land, the test they applied was the same as that applied by the Court of Exchequer in a Warwickshire tithe dispute: is it ridge and furrow?). In 1786 the meadow was surveyed, not assessed, and it proved to lie partly in blocks (divided into sixteen), partly to be grass-growing strips, and partly to be the odd grassland of hollows, corners and slopes among the furlongs where the plough did not go.

In the second village the furlongs were more scattered over the parish, with wood and meadow intervening (rather as in Herefordshire). There was one enclosed field. About one sixth of the arable land was in fact out of cultivation altogether and woodland was encroaching, a situation which must have often occurred in English villages after some temporary setback, to be retrieved at the next burst of optimism or at the next increase of mouths to be fed or arms to wield the axe. The distribution of strips within the six-strip furlongs here followed the order of houses around the central village green: if your house was east of the miller's then you had the strip east of his. G. C. Homans pointed out the evidence for this *solskifte* regularity in some English strip lay-outs, and although his evidence has not been received into the canon of English teaching on the subject, there is plenty of evidence in deeds and terriers that English villagers were aware that their strips could be succinctly described as lying between those of the miller and the parson, whatever furlong they were in.

As in the first village, small tofts lay near the farmsteads: these were enclosed fields, and each house at Grøfte had part of the enclosed fields away from the heart of the settlement. The average strip was rather smaller than in the first village (10–12.5 metres). Some reallocation, by which you had a pair of adjacent strips, had taken place.

In 1768 Grøfte had acquired a fourth Field, cut off from its parent by a road, but it was predominantly a meadow area and the field-system was still triple. Some assarting had increased the size and number of the strips but there had been no *egalisering*.

The third village had five farms. There were again three Fields with considerable inequality (75, 59 and 40 ha.) But the biggest field had the poorest soil. The original holding was of two hides, and then a division took place into two and three parts respectively. This had complicated the regularity of the order of strips within the furlongs. Only about 20 per cent of all furlongs had five strips, the number in the other 80 per cent varying between 4 and 37. Here again, the majority of the strips had an area of 1000–3000 square metres. In this village a radical *egalisering* took place between the two surveys. The number of furlongs was increased from 75 to 137 by subdivision; some of the boundaries of the three Fields were shifted. Only in one case did the longitudinal direction of strips in these furlongs change. The farms now held their strips in strict sequence through the furlong (Nos. 1, 2 . . . 5 in order) although No. 1 did not always hold the outermost strip.

From valuations at the two surveys and in 1844, maps have been drawn showing the four qualities of land as contemporaries valued them. There is general congruence between the results. In an Appendix by Werner Christensen, methodical analysis of soil samples are plotted across the maps of the three villages, and it is shown that the highest concentrations of $P_2\ O_5$ follow the same pattern as the valuation maps. The core of land with the highest soluble phosphate was that near the village. This land, the first to be brought under cultivation, had made humus deep enough for the most exacting cereals, whatever the original quality of the land. Village refuse, human, animal and vegetable had added to its quality. The same improvement in quality by the very act of cultivation is shown by the more uniform quality of the 1844 valuation of land recently brought into cultivation. Men were still in the ' short-run ' of the economists' diagrams, when more and more attention to the soil was still giving increasing yields.

The final section of the book describes what in England would be termed the enclosure of the open fields. This English term is wide enough to embrace the Danish process. At Hejninge eight farms were accidentally burned in 1781 and they were rebuilt at the periphery of the village in pairs. The opportunity was taken to cut out block-shaped compact holdings for these farms, although such was the quality of the newer peripheral land that some land in the more fertile core of the Fields was still held in common by the re-built farms. At Grøfte, enclosure threw two of the farmsteads into the lord's estate and their lands were absorbed into the woodland, just as the houses of Stowe or Wimpole or Castle Howard were being absorbed into their parklands. Four other farms were given land radiating from the village in compact blocks. At Snekkerup, enclosure took place in 1794, again with blocks of land radiating from the village farmsteads. In Plates IX and X the whole district (Antvorskov) is examined to see what took place at enclosure. There were 62 villages, 32 of which were enclosed between 1793 and 1801 either by agreement or by administrative officers of the Government. Some had the new holdings radiating from the centre, others had new farms in the centre of their new holdings away from the old village, like the Waterloo Farms of the English Midlands.

Enclosure in Denmark was in progress in Tudor times although it did not gather pace until the latter half of the 18th century. Danish historians are fortunate in having so many extant open field maps. In England there are fewer maps and the pattern of holding which they exhibit is more complex. The land market among big and little peasant proprietors in the 15th century had produced a fluidity which makes it difficult to see much of an original scheme in the scattered holdings of such an early open field map as that of Weston in Weedon (Northants) or in the Field Book of Castle Howard, both Elizabethan. The open fields were, as the Orwins showed, a practical answer to a practical farming problem: how to reward a village labour force for its work in clearing; and how to maintain the cleared land by communal effort. That problem was a European problem, and where our own records are dark we may, within limits, look for illumination across the Channel.

This book has all the quality of a good illuminator. Its maps are beautifully printed on a scale large enough to read. Its presentation of the matter is orderly, and it adds to English enlightenment by a 53-page summary of the book in English and by adding English captions to all the maps and plates. *O si sic omnes !* M. W. BERESFORD.

NOTES ON THE PREHISTORIC METALLURGY OF COPPER AND BRONZE IN THE OLD WORLD. *By* H. H. COGHLAN, *Pitt-Rivers Museum, Oxford,* Occasional Papers on Technology, No. 4. 1951. 15*s*.

Metalworkers have undoubtedly been pioneers in discovering and applying fundamental facts and principles from which our sciences of geology, chemistry and even physics have sprung. But miners, smelters and smiths were illiterate in all societies before the classical Greek—and in many since. So the only way of finding out what chemistry or geology they knew is to find out what knowledge they used; that means what processes they consistently and successfully employed. Only a minute scrutiny of their products, their equipment and the refuse they have left by one with practical experience of the sort of operations involved can recover the outlines of this preliterate knowledge that yet lives implicitly in our systems of sciences. If only as a competent and illustrated compendium of studies already made in this direction Coghlan's book would rank as a solid contribution to the history of science as well as a convenient guide to professional archaeologists. But it is more than this. It includes metallographic analyses of a dozen ancient copper or bronze artifacts carried out by Dr Voce of the Copper Development Association that give illuminating information on the techniques employed and their makers' mastery of these. Coghlan's explanations render these valuable documents easily intelligible to readers who may be unfamiliar with technical terms and their implications. But the most original part of the book is the account of practical experiments by the author in conjunction with Dr Voce at reproducing procedures supposedly employed by prehistoric metal-workers. He has cast a shaft-hole axe of unalloyed copper in a closed mould, cut copper rods with stone chisels and made successful castings in a prehistoric bronze mould such as armchair antiquaries thought could be used only for making the wax models for *cire perdue* castings.

The book is completed by an extensive bibliography (from which Goetze's articles are curiously omitted) and helpful ethnographic notes by Mr Penniman.

Besides thus surveying the tools and techniques of preliterate craftsmen, Mr Coghlan attempts to reconstruct the steps by which this applied science was accumulated. Like other authorities, he assumes that native copper was known before copper smelted from ores and that oxidized ores (such as malachite) were smelted before sulphide ores that can only be reduced after a preliminary roasting in which the sulphur is burnt away as sulphur dioxide gas. The second assumption sounds likely both because oxidized ores can be reduced so much more simply by merely heating with charcoal and because they are often conspicuous by reason of their colours. It is not, however, true that ' sulphide ores are usually found at a very considerable depth below the surface ' (p. 15). For example in the case of the Ergani mine on the Upper Tigris in Eastern Turkey which was probably worked before local history began, a report just published states that 'there never was a zone of oxidization in this ore body' which consists of copper pyrites (sulphide). So, the only ore body in Central Europe demonstrably mined in prehistoric times—that of the Austrian Alps—consists throughout of sulphide ore even to the outcrop. Of course the exploitation of these Alpine lodes need not have begun much before 1000 B.C., but the sulphur content of 'bronzes' from Troy and other much earlier sites does suggest, whatever Coghlan says, that the treatment of sulphide ores had been fully mastered by the 3rd millennium in the Near East. It should be recalled that sulphur is not one of the 'impurities' revealed by spectrographic (as against the older chemical) analyses and may therefore have been present more often and in larger quantities than recently published analyses (being mainly spectrographic) show.

As regards the development of techniques Coghlan proposes to distinguish four stages: A, the shaping of native copper by forging (hammering and annealing) only (i.e. the discovery that copper is malleable).

B, the production of simple castings in open moulds from native copper (the discovery that native copper is fusible).

C, the smelting of copper from ores and the use of composite moulds.

D, the transition to bronze and other alloys.

The first peculiarity of this classification is the important rôle it assigns to native copper. Previous writers have equated 'intelligent metallurgy' with the recognition that copper is fusible as well as malleable and have been at least prepared to admit that this discovery may have coincided with the discovery of smelting. Unfortunately, while melted copper can be distinguished by metallographic examination from what is merely forged, the distinction of native from smelted copper is far less easy. A second peculiarity of Coghlan's classification might be designed to avoid this difficulty. For with stage B he introduces a second base of division—the form of the product as well as the origin of the material. So Sialk III is taken as a representative of 'stage B'; yet it yielded a shaft-hole adze that, as far as the method of manufacture is concerned, does not differ from the Early Sumerian types so familiar now from Kish, Ur, Lagash, etc. Now it is a fact that the Early Sumerian types of shaft-hole axes and adzes can perfectly well be cast in one-piece clay moulds like that for an axe-adze from Hissar III though curiously enough no one—not even Mr Coghlan—seems to have noticed this. Still these highly developed tools have always and rightly been taken as evidence for the advanced metallurgy of the Sumerians and were in many cases demonstrably made from alloys and therefore assignable to our author's 'stage D'. Moreover, Sialk III, 4 is not much, if at all, later than the temple hoard from Uruk-Warka (Erech) that comprised figures made from an alloy of copper and lead and other articles illustrating a complete mastery of abstruse metallurgical techniques such as cupellation. Hence without very accurate analyses, Sialk III does not constitute a very convincing case of 'stage B'. So in Gawra VIII, here with reserve assigned to 'stage A', Tobler's volume, published only after the printing of this book, shows copper to have been more plentiful than appeared from the sources available to Coghlan. Gawra VIII is in fact slightly later than Sialk III and belongs to a time when the use of metal for large objects like vases and of alloys proclaims stage C or stage D only a little further down the Tigris. The Egyptian evidence is little better. The Badarian and Amratian copper objects are admittedly not products of intelligent metallurgy and therefore can illustrate 'stage A'; a single Gerzean axe-head from Armant is claimed as representing 'stage B', but, as metallic silver and lead were probably then already known, it too might be made from copper smelted from Sinaitic ore. In fact it is only in the old Hungary that a period when native copper was intelligently cast can be accepted as proven, but such a localized industry alone is not evidence for a general stage in metallurgical development equivalent to Coghlan's ' stage B '.

Indeed Coghlan himself, though he sometimes writes as if it had been established that the fusibility of copper had been discovered before the extraction of the metal from its ores, frankly admits that far more numerous and detailed chemical and metallographic analyses are needed to prove the existence of his ' stage B '. He therefore concludes with an eloquent appeal to museum curators to allow more specimens to be thus examined, explaining what valuable scientific results can be obtained without perceptible damage to the specimens. Dr Voce's analyses here reproduced admirably illustrate the point, but the appeal would have been more convincing had the figured specimens been photographed after the notches had been filled in—only a footnote assures sceptics that this has been successfully done.

Of course chemical analyses can provide other historical information no less significant. The impurities found in any smelted (as opposed to electrolytic) copper, help to locate the ores from which the metal has been extracted and can thus provide evidence for preliterate trade comparable to that derived with such striking results from the petrological examination of stone axes. Professor Pittioni of Vienna has discovered a supplemental use for such data—the determination of the chronological range of prehistoric mining. During the last twenty years, in conjunction with the mining engineers Preuschen and Zschocke, Pittioni has been investigating the prehistoric mines in the Austrian Alps. The exciting results of these investigations formed the subject of his lectures to the University of London in 1950, now published in the *Annual Report* of the Institute of Archaeology. Fascinating details have been recovered on the techniques and lives of the Alpine miners and even on the structure of the industry. But its duration was a little vague. Mining during the Late Bronze Age (E=Urnfield period) is indeed well established by pottery and pins from the tailings dumps. Copper slag used to temper Early Bronze Age sherds from valley settlements suggested smelting in that period too, but the settlements in question are so far removed from the mountain mining sites as to raise doubts whether the ore smelted in them were not derived from some other source, perhaps lumps collected from river gravels. On the other hand some sherds from the dumps were reminiscent of Hallstatt (C) forms and inspired Pittioni to ask whether exploitation did not continue into the First Iron Age. Having in the Alpine sites an ore of known chemical composition but a shortage of typologically significant relics, he looked for typologically dated objects made from his Alpine ores and publishes the results in Vol. V, No. 4 of the *Mitteilungen der prähistorischen Kommission der Akademie der Wissenschaften* in Vienna.*

He persuaded the enlightened directors of the Prehistoric Section of the Kulturhistorisches Museum, Vienna, and of the Hallstatt Museum to offer 75 distinctive artifacts from the famous cemetery for spectrographic examination. The analyses were carried out in 1947 by Dr H. Pesta who ten years previously had examined samples of ore from the Alpine mining sites.

An incidental but positive, if surprising, result is that, judged by the impurities, the objects hitherto accepted as imports from Italy seem to have been made from the same sort of metal (i.e. metal from the same kind of ores) as the admittedly Central European

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* ' Spektralanalytische Untersuchungen von Bronzen aus Hallstatt, O.-Ö. Ein Beitrag zum Problem der Chronologie des urzeitlichen Kupferbergwesens in Österreich ' von Richard Pittioni unter Mitarbeit von Hans Pesta and ' Metallographische Untersuchungen an Bronzen aus Hallstatt, O.-Ö.', von Friedrich Morton, pp. 24, V plates, Vienna, 1949, Ös. 13.50.

Hallstatt types. This is true for instance of the celebrated pail cover from grave 696 decorated with embossed animal figures, though a technical and stylistic reconsideration of the lid leaves Pittioni still convinced that the pail is a Venetic product. Either East Alpine copper was exported to Este or the local smiths drew on some unidentified source of very similar ore.

In the rest of the Hallstatt bronzes the impurities detected—notably the presence of traces of nickel, and lead and more minute indications of arsenic and antimony—agree very well with those characteristic of East Alpine ores. An exception is constituted by traces of zinc in 40 per cent of the artifacts examined, an element of which no trace is found in the copper ores. Pittioni, however, suggests that this impurity may be derived from the tin which was alloyed with the copper. But, of course, this possibility draws attention to a difficulty in the determination of the sources of all alloys as against unalloyed copper. In any case until a larger range of ores have been examined for instance from Hungary and the Balkans, with negative results, it cannot be confidently inferred that the East Alpine lodes did in fact supply the copper for the Hallstatt bronzes. With this reservation the figures here published do enhance the probability that mining in the Eastern Alps did continue into the Hallstatt period. V. GORDON CHILDE.

FORSCHUNGEN AUF KRETA, 1942, ed. F. MATZ. *Berlin: de Gruyters*, 1951. *pp.* VIII + 166, 122 *plates*. DM. 80.

During their occupation of Crete members of the German forces and other German archaeologists were able to carry out excavations at a number of points. The results of these excavations are now published in this large and generously illustrated volume. In a foreword the editor regrets that these war-time excavations had to be left unfinished, and also that circumstances did not permit the Greek Archaeological Service to take cognizance of them. The latter regret will be echoed by all those who, in Crete or elsewhere, have had the fruitful experience of co-operation with Greek colleagues. This said, it must be added that the work was carried out in accordance with the best traditions of German scholarship, and that this book forms an impressive record of work done in time of war. It is to the credit of these Germans that, in spite of their difficult position as members of the occupying forces, most of them established good relations with their Greek workmen. After the war I was told by one of these workmen the story of the excavation of a cave in which some arms had been hidden by the resistance movement, and the blind eye turned by the German excavator to the hasty attempts of his workmen to conceal the rifles: also of his reception at the neighbouring monastery, which had been able to offer him only lenten fare before this incident but, when he returned in the evening after it, had mysteriously found a roast kid and some old wine.

The main interest of these researches lies in the light thrown on the early history of Western Crete, about which, as a glance at Pendlebury's *Archaeology of Crete* and its maps will show, very little was known before the war. The excavation of the Kumarospelio cave in the Akroteri near Khania has produced the first neolithic settlement known in the western half of Crete. The surviving vases from the Late Minoan cemeteries of Kydonia and other finds from the city of Khania have been arranged by Jantzen in the Museum in the little mosque by the harbour, which was founded as part of the wartime operations here described. Their publication makes possible for the first time an archaeological commentary on the importance of the city of Kydonia.

The following periods, Protogeometric and Early Geometric, are represented by sherds from a sanctuary at Aptara (only a few of which could be illustrated). For those who prefer the fuller light of the classical period, there is an account of the large but hitherto not much studied city of Aptara, and the report of the excavation of the Hadrianic Diktynnaion in the far west of the island.

The main excavations recorded in this book are those of a ' tholos ' and M.M. settlement at Apesokari in the Mesara, and a Palace at Monasteraki to the west of Mt Ida. Nearly half the book is occupied with Kirsten's account of his excavation at Monasteraki, and his valuable discussion of the history of settlement in Western Crete. The ' Palace ' of Monasteraki is so identified from its extent and in particular from the area covered by the magazines, with pithoi still in position. Only about a quarter of it was cleared, and yielded neither structure of architectural importance nor many attractive finds ; the most interesting are the pithoi with their sealings. Three building periods are distinguished, all falling within the M.M. period. The importance of the site is above all historical ; that here, for the first time in that half of Crete which lies west of Mt. Ida, a ' palatial ' centre has been found. The contemporary site at Apodoulou lower down the valley of the Platypotamos, excavated by Marinatos and further explored by Kirsten, should belong to this centre. The valley is a rich one and, though it now lies off the main roads, the lowest crossing of the island from south to north, except the Isthmus of Hierapetra far to the east, lies at its head. By this route, Kirsten holds, Minoan civilization spread from the Mesara to the north coast in the neighbourhood of Rethymnos ; and at least as early as M.M.I., not, as commonly held, much later. It is worth observing that Pendlebury had foretold that evidence of this expansion would come to light in this area (*Archaeology of Crete*, 281). There are other sites farther west where, as Kirsten says, it would be worth looking for palaces.

No L.M. remains were found at Monasteraki. After a gap the record of this region begins again on a new site, with the occupation in L.M. III of the lofty acropolis of Sybrita, which controls the passage of the watershed by the south-north route just mentioned. Soundings were made at a number of points within the area of the ancient Sybrita which illustrate the history of this, one of the most extensive cities of Crete. Its most flourishing periods, to judge from the finds, were the Hellenistic and Roman (3rd century A.D.).

These are the first excavations made by German scholars in Crete. They were conducted in not the happiest circumstances and are at many points incomplete. But they are well worth while both in positive results gained and in suggestive ideas which call for further research. T. J. DUNBABIN.

THE DEAD SEA MANUAL OF DISCIPLINE : translation and notes by W. H. BROWNLEE. (*American Schools of Oriental Research* ; *Supplementary Studies Nos.* 10–12). *Drawer* 93 *A, Yale Station, New Haven, Conn.* 1951. $2.00.

BIBLICAL INTERPRETATION AMONG THE SECTARIES OF THE DEAD SEA SCROLLS. *By* W. H. BROWNLEE. *The Biblical Archaeologist*, vol. XIV (*Sept.* 1951), 54–76.

Since the discovery of the Scrolls in 1950 we have all been eagerly awaiting the publication of some of the material. The first item at the head of this review is a first instalment, appearing under the editorial aegis of Professor Albright. His name and the author's are a sufficient guarantee of meticulous scholarship, and no more need therefore be said on that count. (The reference on the first page of the *Biblical Archaeologist* to the ' Bodleian Library at Cambridge ' is rather disconcerting to an Oxonian, but is doubtless merely a slip of the pen).

The Scrolls were probably deposited in the cave where they were found either in the 2nd or the early 1st century B.C. The Manual of Discipline is regarded as belonging to a community known as Essenes, and to that sect of them which was allowed to marry. It contains the Rule of the Community, and is regarded by Professor Albright as 'unquestionably the most valuable of all the scrolls yet known from the standpoint of history'. That appraisal Professor Brownlee regards as 'justified, if one thinks not of historical data as events (of which there is almost no record in this scroll), but rather of religious data which illumine our understanding of pre-Christian Judaism, and which bring fresh light to bear upon the background and thought of the New Testament'. Thus regarded, the Manual reveals a community whose conduct was based upon the general observance of rules, combined with a hatred of deviationists (called the 'sons of darkness') and of 'all the men of Belial's lot' who are comprehensively 'cursed without compassion' and 'damned in the gloom of eternal fire'. It is nowhere revealed what these unfortunate people have done to deserve such execration; the language used throughout is obscure, consisting of platitudinous metaphors about the sons of light and darkness, righteousness and sin, truth and falsehood, cleanness and uncleanness, purity and impurity, without any attempt at defining what these things connote in action. One has the impression of a group of 'unco' guid' who regarded all outside their ranks as damned. Such groups are a common phenomenon of human history. They usually take their stand upon the blind and literal interpretation of a Holy Book or collection of writings. To what extent this 'bibliolatry' had developed is well brought out by Professor Brownlee's second article; it had become a sort of game akin to cross-word problems and the breaking of codes, and was carried on without interruption in every 'cell' throughout the whole 24 hours by an organization of ten men. Though seldom carried to such extremes, the worship of the written word is still one of the besetting sins of humanity.

Into this musty charnel-house the teachings of the Founder of Christianity must have swept like a hurricane. Though the Essenes regarded the Pharisees as bitter enemies, the denunciations of the Gospels suggest that they differed little from each other. How refreshing to read the Gospel of St. Matthew directly after reading this dreadful Manual! There is a Hellenic clarity and concreteness in its message that contrasts with the fussy intolerance and inhumanity of the Manual as vividly as do the plays of Shakespeare with some dreary medieval disquisition. O.G.S C.

NOTES AND QUERIES ON ANTHROPOLOGY: 6th edition, revised and rewritten by a Committee of the Royal Anthropological Institute of Great Britain and Ireland. *Routledge and Kegan Paul*, 1951. 28*s*.

The bulk of this book is devoted to social Anthropology and modern Material Culture, but Part IV is a brief and inadequate collection of notes on Field Antiquities. Some of them will be found useful by field workers; others, such as the sentence: Metal has long been used: will not. Some are positively misleading, such as the advice not to expose to the air long-buried skeletons 'as they are apt to break up quickly'. The reverse is true; exposure hardens them. There are Appendices on Photography, Cinematography, Collection and Packing, Preservation of Bones, and Paper Squeezes. The caution about the use of a dark cloth when focusing is surely otiose nowadays. The whole of this section and the Appendices need to be scrapped and rewritten by someone with a wide experience of modern archaeological field work. But even so it seems doubtful whether such a vast subject could be usefully dealt with in 26 pages. There is a useful book-list of 17 pages, including 'some periodicals'; of these only some have addresses given. Amongst the omissions is the address of the Royal Anthropological

Institute itself, which is responsible for the book. The address of ANTIQUITY is wrongly given as Gloucester. Attention to important details is what distinguishes useful and scholarly productions from others. O.G.S.C.

EGYPTIAN PREDYNASTIC POTTERY. Otago Museum Handbook, No. 1. 1950. 16 *pp. text, map and* 20 *plates.*

This handbook is planned to inaugurate a series describing collections in the Department of Ceramics at the Otago Museum and is written essentially for the benefit of the layman. The text is by Colonel Fred Waite, supported by half-tone illustrations by Dr D. W. Carmalt Jones, of the characteristic pottery forms of most of the Egyptian Predynastic cultures as exemplified by the Museum collections.

The book is divided into two parts, the first dealing with the different methods of making pottery by hand, and a brief sketch of Predynastic chronology. Petrie's sequence-dates are adopted, but other estimates of dating are given in Appendices I and III to show the by no means inconsiderable differences of opinion that still exist. It is to be hoped that C14 dating will do much to provide a firm chronology for the Predynastic.

The second part gives a brief description of the more important pottery forms found with the different Predynastic cultures commencing with the Fayum. The author seems to suggest that Maadi ' is a primitive rural version of the Fayum Culture ' whereas most Egyptologists would place this site after the Semainean and immediately before the First Dynasty. One might perhaps put forward the contention that a description of pottery forms alone does not give any very clear picture of Predynastic culture. Its pottery is not the thing of intrinsic beauty that Greek or Chinese pottery is, for example, and one would have liked to have seen the description of the pottery against a background of prehistoric predynastic civilisation as a whole. This is partly, though only partly compensated by the illustration of two figurines from Merimde.

The handbook will, however, be very useful as an introduction to Predynastic pottery; it is well produced, and the Otago Museum is to be congratulated on the inauguration of what promises to be a most useful Handbook series. J. DESMOND CLARK.

THE STORY OF PREHISTORIC CIVILISATIONS, by DOROTHY DAVISON. *Published by Watts & Co., London,* 1951. *Price,* 12*s* 6*d.* 266 *pp.,* 65 *black and white illustrations and* 20 *black and white drawings used as chapter headings. Two end paper maps.*

It is difficult to see the class of reader for whom Miss Davison intended her book. A book which is intended for the intelligent, enquiring amateur requires to clothe the bare bones of archaeological data so that they may, in the words of the author's preface, ' become the life and thought of real men and women '. This, however, on her own assertion, the author leaves her readers to do for themselves. In a book of this size (266 pp. 8vo), which treats of a subject so wide as the growth in the Near East and the spread into Europe of neolithic civilisation from the close of the Palaeolithic to the beginning of the period of the henges and the Bronze Age, there is no room for more than a concise summary of each culture or civilisation and the author leaves herself no space for making her subjects come to life. The intelligent amateur needs his interest awakening and stimulating if he is to pursue the subject at all.

The beginner in archaeology, though he be more serious than the amateur, requires more detail in the accounts of the various cultures than this book, limited by its small size and the scope of its subject, is able to supply, and his knowledge is not yet sufficiently wide to enable summaries to be of much practical value. He needs a full meal not an iron ration. As a rule the specialist would turn to specialist works for any detailed information he might require, though this book gives useful outline notes on the Neolithic as a whole. Unfortunately the illustrations, with which the author has profusely supplied her book, would not be as much help to him there as they might. Miss Davison herself deplores the ' overcrowding ' and ' disregard of comparative size ' which have only ' been allowed most reluctantly, because it seemed necessary to draw the greatest possible number of articles in view of the dearth of illustrations in archaeological books and the extreme difficulty of gaining access to the journals which give original accounts of excavations '. It might have been of greater value to have omitted a few of the smaller illustrations, which because of their small size it is difficult to make accurate, in favour of a larger size for the more important ones particularly the maps. It is unfortunate too that in several cases the text to the illustrations is deficient.

While, therefore, the book may not be the ideal introduction to the Neolithic for the amateur, the serious student will find in it one great merit, which like its defects, springs from the wide scope of the work. He will find concisely summarised and in one volume all the main outlines of the Neolithic Civilisation of Europe.

J. DESMOND CLARK.

MAN THE TOOL MAKER, by K. P. OAKLEY. *Published by the British Museum, London.* 1949. *Price, 2s 6d.* 92 *pp.*, 41 *line illustrations*, 2 *plates*, 1 *table, select Bibliography and Index.*

The value of this handbook as an introduction to the study of prehistory cannot be over-stressed ; it is unquestionably the best that has been written in recent years. That is chiefly because the author never neglects the fact that the stone or bone which he is describing is an object made by Early Man, and never loses sight of ' Man ' as ' the Tool Maker ', among the wealth of typological and technological material which the book contains. Throughout the book, whether it be a description of Choukoutien bipolar technique or Levalloisian flaking methods we never lose sight of the all-important fact that these were the tools of man, made as a result of his capacity for logical, conceptual thinking. All too often books of this nature on prehistory have in the past tended to stress the typological differences of the various cultures of the Stone Age. *Man the Toolmaker* leads one to view the subject from the point of view that the differences are the result of traditional development coupled with man's inventiveness.

The handbook treats the subject from a world viewpoint and not merely from that of Western Europe. So often the prehistory of other parts of the world is, as it were, ' tacked on ' to a description of the European sequence ; but Dr Oakley has succeeded in treating of the subject in such a way that the picture obtained by the reader is not a rather restricted view but embraces the development of prehistoric cultures in most parts of the world knit into a whole.

The book follows the usual form in giving an account of the early controversies on the antiquity of man, methods of fracture of stone, the cultural succession and so forth ; but the tale is told so clearly that often controversial points are assimilated by the reader with the minimum of mental labour.

Dr Oakley regards all eoliths with the greatest suspicion and his treatment of this problem is excellent. His chapter on raw materials could be read with considerable benefit by many more advanced in the study of prehistory ; it brings out very well the fact which we are apt to forget, that stone was only one of the many raw materials used by prehistoric man. The description of the making of stone implements is greatly assisted by illustrations of techniques and of the various fabricating tools, hammerstones, compressors, etc., which help to emphasize the importance of technique rather than typology.

The description of the Palaeolithic cultures incorporates the latest ideas on their origins and development and the expert will find much food for useful thought—for example as to the origins of the Levalloisian technique and cultures, or the use of the Stillbay bifaced point.

A particularly valuable chapter deals with the tools that are associated with the various types of fossil man. All too often these are dealt with separately or, as has happened in not a few of the African fossils, the human types have been described but not the industries.

The book concludes with a chapter on the attributes of 'Man' the 'Toolmaker'. The reasons for culture change, primitive art styles and religion and man's control and mastery of his environment and food supply.

A useful glossary and chart showing the Palaeolithic culture traditions and their probable connections complete the book.

Mr Waterhouse's illustrations cannot be bettered and one must admire the care with which the subject matter has been chosen, to supplement so clearly the text; particularly happy is the inclusion of reconstructions of haftings and the photographs of modern stone craftsmen in Australia.

This book provides an introduction to the study of prehistory that has not been bettered and will be of value not only to the amateur but to the expert also.

J. DESMOND CLARK.

MATMAR, THE BRITISH MUSEUM EXPEDITION TO MIDDLE EGYPT, 1929–31. *By* GUY BRUNTON, O.B.E. *Quaritch, London,* 1948. *Pages* V *and* 107 *and* 76 *plates. Price not stated.*

Death withheld his hand just long enough to enable Guy Brunton to see through the press his last excavation report, delayed some seventeen years by various causes, chief of which was the Second World War. Readers of ANTIQUITY should be particularly gratified that the work of this expedition has not been lost but recorded in Brunton's usual meticulous way, and illustrated by Winifred Brunton's beautiful drawings. For it was an appeal circulated in ANTIQUITY which brought a considerable measure of financial support to the expedition, the chief benefactor being Mr J. A. Roebling of New Jersey.

Matmar covers two seasons' work of exploration and excavation along the edge of the eastern desert north of that part of the Badari district of Middle Egypt which had been explored in 1928 and 1929 under the auspices of the British Museum and published under the title of *Mostagedda.* Its aim was to find more remains of the predynastic and earlier periods.

It left the Tasian culture still doubtful, and added little to our knowledge of the Badarian, although it found the best Badarian bowl yet known (an unusual one in that its rippling is vertical) and produced both a perforated shell fish-hook and a notched ivory one. From predynastic graves came one of the rare bowls with animals modelled in the round (in this case hippopotami and crocodiles) on the rim, that probably have an Asiatic inspiration, and the earliest heavy copper tool yet known, a fine axehead of S.D. 38–46 with a high nickel content.

There were as usual also finds from various historic periods. The Fourth to Tenth Dynasties and the Second Intermediate Period provide useful supplements to the Corpora of finds from those periods already compiled in Brunton's earlier reports *Qau and Badari* and *Mostagedda.* From the New Kingdom traces were found of temples built by Akhenaten and Rameses II, the latter in honour of Set, associated with which was a bone pit similar to those found at Qau where fossil bones as well as worked hippopotamus ivory had been interred, that animal being sacred to Set. And the discovery of an intact, though poor, cemetery dating roughly from 950 to 650 B.C. provided a large series of beads and amulets, and especially scarabs and plaques, belonging to the 22nd to 26th dynasties, a period not hitherto closely defined.

Matmar closes the account of the work of a great Egyptologist, except for a study of button seals, and it is greatly to be hoped that the material that he left for that will find an editor.

For the thoroughness with which he carried out his master's principles of making the smaller objects in Egyptian archaeology provide the dates on which the reconstruction of prehistory and history are based, Brunton should be recognized as Petrie's greatest pupil. Not only did he extend our knowledge of the Predynastic backward by describing the Badarian period, but his development of the Corpus system which Petrie invented has provided a body of information about the pots, stone vases, beads, amulets, scarabs, etc., of the historic periods which, although it may be added to, will never be replaced.

A. J. ARKELL.

SAALBURG JAHRBUCH X, 1951. *Edited by* HANS SCHÖNBERGER. *Walter de Gruyter and Co., Berlin.* 80 *pp.*, 5 *plates and* 2 *large plans. Price not stated.*

The reappearance of the Saalburg Jahrbuch, after an interval of twelve years, is to be welcomed most warmly. The quality of paper, illustrations—and contents too—is well up to the old standards, though one misses the convenient binding in boards. In a brief introductory note, the editor announces a widening in scope; in future the series will contain papers relating to Roman provincial archaeology and frontier studies, on prehistory and Dark Age archaeology generally, besides continuing to report on work in those fields within the area served by the Saalburg Museum itself. Next comes an obituary of Heinrich Jacobi, who died in 1946 in his eightieth year, first the helper and then the successor of his father, Louis Jacobi, in the creation of that splendid museum, whose journal he founded in 1911; a bibliography of his writings, from 1893 onwards, is appended. There follow half a dozen papers: Josef Baas discusses the different kinds of fruit eaten in Roman times at the Saalburg, identifiable by their stones, and gives a useful bibliography of French, German, Swedish and Swiss studies in the same field; Heinrich Bingemer studies a rectangular earthwork near Ohrenbach, in the hills west of Miltenberg, and makes out a case for Roman origin, yet to be tested by the spade; Wolfgang Dehn gives an essay, in honour of Emil Krüger's eightieth birthday, on the Gallic *oppida* as described by Caesar and other writers (this might have been more at home in a periodical devoted to history rather than to archaeology, but it will undoubtedly contain much of interest to the archaeologist); Werner Jorns reports on the excavations at Zugmantel in 1935, and Hans Schönberger surveys the whole series of excavations at that important site, up to 1950. A very large folding inset gives on one side a plan of Zugmantel forts and external settlement, on the other a plan to a still larger scale of one small portion of the latter, excavated in 1935–6 and 1949–50; particular interest attaches to the increasing evidence that the cellars there, and in the similar settlement at the Saalburg, belonged to long, rectangular buildings whose gable ends fronted on to the main roads, as in the case of Housesteads on Hadrian's Wall or at Corbridge, though the British examples have not yet been found to include such cellars. Finally, Helmut Schoppa contributes a paper on some Roman wells found in Cologne. Altogether, it is a very interesting mixture, and we shall look forward with great interest to further issues in the series, which it is hoped to make an annual one. Eric Birley.

ETRUSCAN VASE-PAINTING. *By* J. D. Beazley. *4to, pp.* xvi, 351, *with* 40 *plates. Oxford: at the Clarendon Press.* 1947. 84*s net.*

In this handsome volume Sir J. D. Beazley inaugurates a new series of Oxford Monographs on Classical Archaeology of which he and Dr Paul Jacobsthal will be the general editors. Since the advance of scholarship banished the term 'Etruscan', once applied to Greek painted vases in general, from the place which it had usurped, the actual Etruscan vases, of which there are many, have been somewhat neglected, and this work by the acknowledged master in the field of Greek vase-painting is the first comprehensive study of them. With minor exceptions, such as the earliest products of Etruscan or Italic vase-painting of the 8th or 7th centuries B.C., all types of decorated vases are studied. Rather more space is devoted to red-figured than to black-figured ware, which has received more attention elsewhere, but special care is given to black-glaze vases, which form a large proportion of the finds on Etruscan and early Latin sites. Other types included are vases with superposed colour, those with patterns only, and silvered vases. The book, with its documented descriptions of the vases and their assignment to their places of origin, or even, within limits, to individual painters, is a mine of information to the specialist, but also has a strong appeal to a wider public. Students of Greek and of the fine Arts will find much to delight them in the detailed examination of the subject matter of the pictures and their style. The author's incomparable knowledge of Greek vases enables him frequently to identify the Greek types which inspired the work of the Etruscan artist, and with a subtle sensitiveness to style he reveals, often in a masterly phrase, the elements in the Etruscan work which are characteristic and original, although the general conception may be derivative. Thus of a cup from Vulci, figured in Pl. xii, of which the interior is decorated by a winged figure (Zetun), carrying off a woman (Phuipa), with a general resemblance to extant Greek designs with Eros similarly portrayed, Professor Beazley remarks: 'Here again we notice the sense of effort, as if the winged one were trudging with his burden through heavy soil, and this sense is increased by the clogging garment, the blown hair, the starting eyeballs, and the open panting mouth. If this is Greek it is Greek spoken with a strong accent'.

With similar discernment Professor Beazley recognises in a scene from the adventures of the Argonauts, depicted on a stamnos in Boston, the hand that rendered a 'Zeus and Ganymede' picture on a similar vase at Oxford. There is a justifiable note of triumph in his words, when he says: 'Ever since this vase came to Oxford, nearly thirty years ago, I have wanted to find another work by the same master: and here is one'.

Of the decoration of a stamnos in the British Museum he writes: 'On the other side a rider; Eros bringing him a wreath and sash in token of victory, and another Lasa (an Etruscan goddess or daimon, with wings) sitting on the ground, smelling a flower and not heeding the others. These pictures remind one of a pagan scene by some very humble Italian painter of the 15th century: something between a vision and a Valentine'.

Professor Beazley's introductory chapter is full of apt descriptions that show a fine appreciation of the nuances of style and subject matter. In a portrayal of the sack of

Troy on a calyx-krater in the Villa Giulia, ' Helen, in a himation only, trips away mincingly, with much white showing, and a perfect air of injured innocence '. Of the remarkable cup by the Settecamini painter in the Cabinet des Médailles, with a unique subject, Pasiphae, nursing the infant Minotaur, he says : ' The delightful little fellow has passed his hand round his mother's waist, between the two garments, where it is warm. Several episodes from the story are represented in antiquity. Etruscan cinerary urns of the Hellenistic period show Minos when he first sets eyes on the infant—a tense moment. In Euripides' play we hear him raging, and hear Pasiphae's tremendous defence. Then there is silence : but after all the Minotaur survives. Someone gave him a beautiful name Asterios ; and someone must have reared him : why not his mother ? Here is a port-hole view, as it were, of an otherwise obscure period in the life of one, whom some at least of my readers would probably describe as their favourite character in fiction '.

In addition to well known vases, such as the attractive one in the Villa Giulia with a war-elephant, surmounted by a tower and followed by a baby elephant, which has been associated with the campaigns of Pyrrhus, the plates contain much unpublished material. The illustrations and detailed descriptions embrace the whole range of subjects found in Etruscan vase-painting, daemons, monsters, funerary, mythological and battle scenes, and throw some light on the thought and culture of that somewhat mysterious people.

This first volume of the new series with its elegant printing and fine plates is a worthy product of its famous press and in it Professor Beazley sets a high standard for his successors. G.F.F.

DIE SCHLESWIG-HOLSTEINISCHE KNICKLANDSCHAFT. *By* GUNTHER MARQUARDT. (*Schriften des Geographischen Instituts der Universität Kiel, Vol.* XIII, *no.* 3. *Kiel,* 1950). *pp.* x+90, *with* 29 *photographs and* 10 *maps in appendix. D.M.* 6.50.

The ' Knick ', whose contribution to the Schleswig-Holstein landscape forms the subject of this study, is a boundary-hedge raised on a mound. It is named from the knicking or cutting of the growing bushes or trees which is also a feature of English hedging, and in Plate 6 the author stands affectionately by an Oxfordshire *Knick.*

The author's interest is in the distribution and origin of this form of hedging, and he appears to have a practical interest in their advocacy as a protection from winds and strong sun. To explain them he invokes weather, soil and economic history ('Klima, Boden und Mensch prägen das Aussehen der Knicklandschaft '). Protection from winds encouraged their construction ; the soil determines whether they shall bear hazel or birch on their crowns ; the history of land-reclamation and land-use has determined their shape and position.

A full analytical contents page, profusion of maps and 29 photographs make it easier for the reader with only a little German to follow the argument, see the evidence and ponder its significance for our own landscape. Why might a study prompted by Dr Marquardt's techniques be interesting on this side of the North Sea ?

All over Europe the hedge stands for a particular form of land-ownership and a particular technique of cultivation, and neither of these have been universal in the past. Whatever the crop and whatever the language of the man sowing within the hedge, it is very unlikely that anyone except the sower (or his master) decides what should be sown, how it should be tended, and when it should be gathered. The significance of the hedge field is that it erects a sign to all except one man, and the sign says : Keep Out.

There was a time, however, when fields bore a very urgent sign which read: Come In; since their creation, maintenance, ploughing and manuring depended very much on other men and their animals *not* keeping out.

Our knowledge of the pace and place of enclosure in England is beginning to be detailed enough for us to be more particular, and to speak of enclosure-when and enclosure-where. The hedge arrived at different times and for different purposes. Land coming straight into cultivation at a rather late date, and in country not tempting to earlier settlers had every chance of going straight into the individually-hedged field and by-passing the open-field stage which persisted in some villages until Victorian times.

Dr Marquardt's lesson to us should be that the hedges themselves, if carefully examined and their distribution mapped, might afford another clue to enclosure-when. Such a study would also make us more particular about speaking of the English hedge in general terms, instead of the hedge-what-of, the hedge-where and the hedge-when. Field-study would enable us to dispose of the first two questions, but only a close marriage of field-work and documents would enable us to fit hedge-types to periods of enclosure in various districts; and thus to use hedge-types and field-shapes as clues to the dating of enclosure where documents have not survived. Dr Hoskins and Mr Finberg have recently done something to give historical explanation to the deep-sunken Devon lanes and their sturdy, high-banked *Knicken* hedges. The Midland field, drawn out by the surveyors' pencil at enclosure and hedged or fenced according to the best advice of the 18th century agricultural hand-books, is another clear type. The large, irregular-shaped field of the 16th century enclosures for pasture can also be seen on the 6-inch Ordnance map by its very individuality among its rectangular neighbours. There are many such on the Buckinghamshire landscape.

If other books like this one from the Continent can set us thinking about and looking at our own historical landscape they will not be a luxury import. M. W. BERESFORD.

BAROQUE AND ROCOCO IN LATIN AMERICA. *By* PÁL KELEMEN. *pp.* XII, 302, 192 *plates. Macmillan, New York,* 1951. $16.50.

The most abundant material for a study of this kind is to be found in the churches and this book deals primarily with them and their furnishings, although houses are not excluded. Retables, images, pictures, organs, stalls, benches, cupboards and related objects are included in it, but not silver or textiles. The bibliography gives an idea of the extent of the previous work on limited areas, but this is the first attempt to survey Colonial art as a whole. It has resulted in an appreciation of the great variety of the ways in which Indian and Mestizo workmen from different places reacted to stimuli from Europe, with the result that many local styles developed within the Baroque pattern. That such variations existed has long been known, an obvious instance is the difference between Cuzco and Arequipa both in Southern Peru, but their extent has been forcibly demonstrated by Mr Kelemen. The work has involved much travel, and no little hardship, as anyone will realise who has left the beaten track in Latin America. The book is beautifully produced, and the illustrations, many of them from photographs by the author's wife, would by themselves make it a prized possession.

After introductory chapters entitled 'The Colonial Scene' and 'The Artistic Climate', which set the scene, and one on the Cathedrals, which is a preliminary canter over the field, there follows one dealing with representations of our Lord and the Cross, called 'Christ in the New World'. Most of the following chapters treat the subject geographically, but the sequence is interrupted by two on sculpture and statuary, one general and the other on the outstanding school of Quito, separated, not quite logically, by a chapter on the buildings of Central America called 'Earthquake Baroque'. Two chapters, on painting and on organs, precede the final regional chapter on Brazilian Rococo, which is reasonable since they treat principally of countries other than Brazil. On the other hand, a special chapter on woodcarving, 'The Woodcarvers' Language', covers the whole field and falls well into place at the end.

Many of the illustrations are taken from objects not previously published, which increases our debt to the author. Among them is one of the finest things in the book, namely Plate 79, the figure in the *camarín* behind the high altar of San Francisco at Popayán, Colombia, which is also shown on the dust cover. This seems to represent a fusion of the Immaculate Conception with the Woman in Chapter 12 of the Apocalypse, as the author reasonably suggests, but it is worth pointing out that the captions to this and the following plates, saying that they represent the Visions of the Fifth and Sixth Seals, do not refer to the seals in Revelation Chapter 6 as one might expect, but to what the author calls 'mystic lore'.

Travellers in Latin America will know what a wealth of this art is to be seen there, but the forces of destruction are many and only too successful, as Mr Kelemen points out. We already owe him a great debt for the pleasure this book will give; it will be multiplied many times if his efforts serve to awaken Church and People in Latin America to the value of their heritage. G. H. S. BUSHNELL.

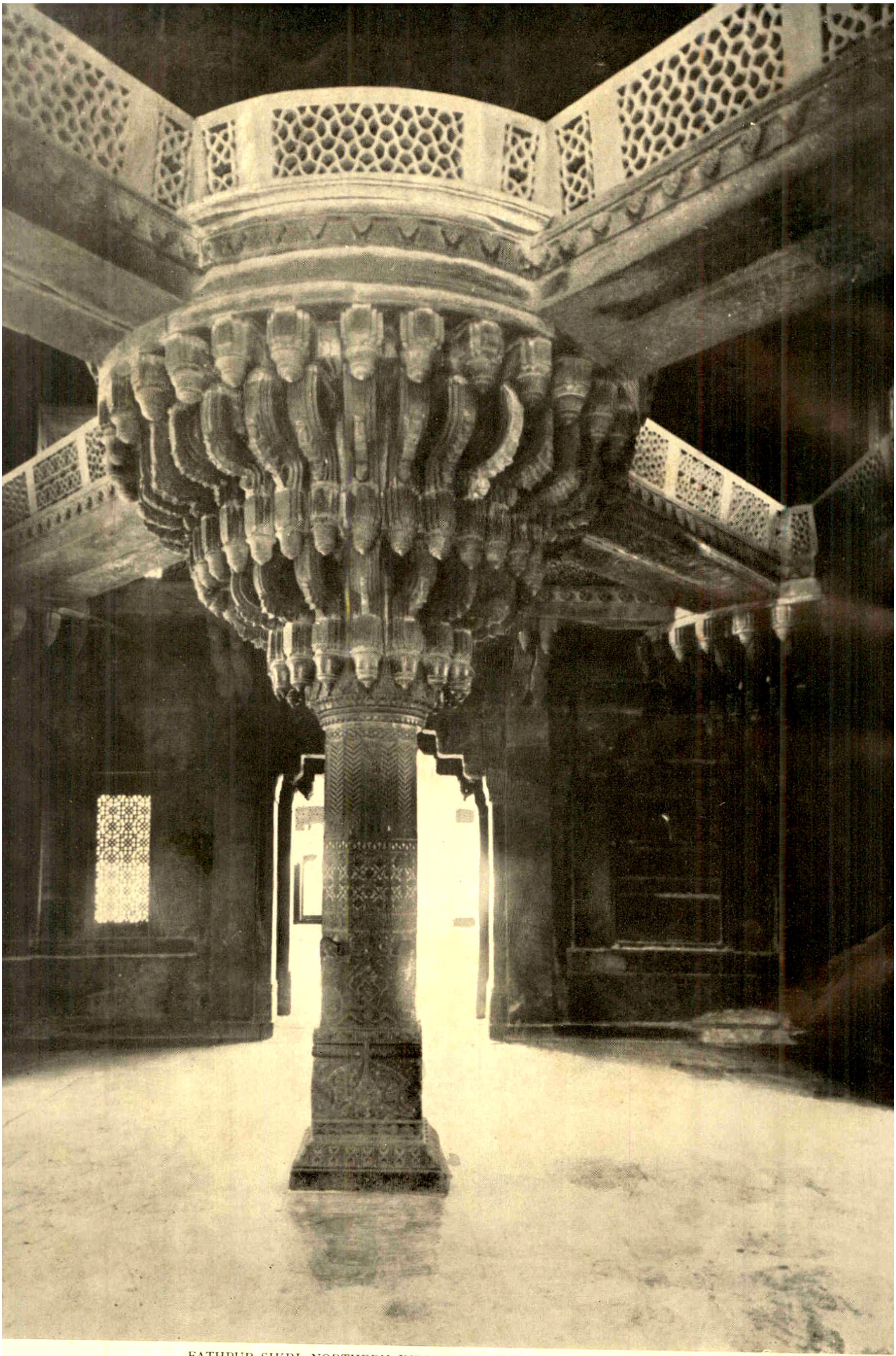
THE EARLY CULTURES OF NORTH-WEST EUROPE. Twenty-two Essays written in honour of H. M. Chadwick. *Edited by* SIR CYRIL FOX *and* BRUCE DICKINS. *Cambridge, University Press*, 1950. XVI+442 *pp., with* 17 *text figures and* 12 *plates*. 63*s*.

These essays range over the field in which the late Professor Chadwick himself was interested. The emphasis lies, naturally, on the archaeology and early history of these islands, in particular on Anglo-Saxon England, and on British relations with the Scandinavian north. The earliest, a study of the Long Barrow in Western Europe, by Mr Glyn Daniel, carries us back to the late Neolithic. The latest, by Mr C. E. Wright, is concerned with a 17th century antiquary, Sir Edward Dering and his 'Saxon' charters. No reviewer less learned than the scholar in whose honour the volume was compiled could hope to do justice to the varied fare provided; it must suffice to note and comment on some of the contents.

Eight papers are classed as archaeological. In this section the most notable contribution is Mr de Navarro's study of the British Isles and the beginning of the Northern Early Bronze Age. In a fully documented discussion of the material he brings out the part of these islands in the formation of this culture and illustrates the reflex influence of Scandinavia on Britain. In absolute terms he dates the Northern Early Bronze Age from between *c.* 1425 and *c.* 1400 B.C. down to 1300 or slightly later. Miss Clare Fell contributes an admirable regional survey of the beakers found in Cumberland and Westmorland. Sir Cyril Fox has assembled from his detailed reports on barrows in South Wales the evidence bearing on the ritual used in the construction, a subject too long neglected by excavators. His plea for the total exploration of royal burial mounds should not be neglected. The Norse Heritage of the Isle of Man is treated by Mr and Mrs Megaw, who indicate the need for re-examining accepted views on some of the early Christian remains of the island.

The rubric Celtic is headed by an article in which Mr T. G. Powell attempts, not altogether convincingly, to correlate the archaeological evidence with literary traditions of the Celtic settlement of Ireland. His survey clears away many misconceptions, but the material is hardly ripe for so far-reaching a reconstruction. Mr Kenneth Jackson discusses the Ogham inscriptions of Southern Britain and their dating. He would place the ' bilingual ' stones between the mid-5th and early 7th century. In a footnote he criticizes the weight laid on the Cadfan stone as a criterion of date, but his conception of the successors of a powerful king, who ' might easily have ordered the very newest decorative lettering from the Continent ' does not ring true to the conditions of Celtic society in the 7th century.

Anglo-Saxon studies claim a further eight papers. Following a clue left by the late Max Förster, Professor Bruce Dickins produces the missing part of a manumission in the Exeter Book from Leofric's Gospels in the Cambridge University Library (Ii, 2, 11). The Moore memoranda on Northumbrian History are discussed by Mr Hunter Blair and the post-Bedan Miracles and Translation of St. Cuthbert by Mr B. Colgrave. Literary studies are contributed by Miss D. Whitelock, who provides a new interpretation of the Seafarer, and Miss J. I. Young. Mr Bromwich argues cogently in favour of the Alfredian authorship of the prose portion of the Paris Psalter. Finally we may note two articles on Norse studies and one on the Fenland Place Names. C.A.R.R.



FATHPUR SIKRI, NORTHERN INDIA, 1570. THE DIWAN-I-KHAS

he centre of the high, vaulted chamber stands a single large column with a huge capital

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Antiquity

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Editorial Notes

IN September we printed eight more pages than usual in ANTIQUITY and we do so again in this number. We now print *more* review-matter in each number than before the war; and although thinner paper makes the size look smaller, the total contents of ANTIQUITY are only less by one-third than the pre-war issues. The increase in price (to 30 shillings) is generally agreed by our subscribers to be both just and necessary, and we are glad to say that our circulation has hardly been affected by it. If ANTIQUITY were as necessary to life as are food and clothing we should be charging about twice as much for it, and getting it, as one of our readers pointed out to us. But we are able to survive and flourish, and there is no lowering of the standard. We once more thank those who, by their continued support, enable us to achieve this satisfactory result, reminding them that early renewals of their subscriptions (now due) are greatly appreciated.

* * *

The Editor has had several letters enquiring about the results of his Sudan expedition at the beginning of this year. These results will be published by the Sudan Government, but he hopes to print an account also in ANTIQUITY. It will be based upon a lecture which he is giving to the British Academy on December 10th next (5.0 p.m.); admission to the lecture is by invitation, but we understand that, provided there is room, tickets may be available on application to the assistant-secretary of the British Academy (Burlington Gardens, London, W.1). It will be illustrated by lantern-slides made from the photographs taken and the plans made. The chief subjects dealt with will be neolithic rock-pictures and the castles and churches of the Sudan (Middle Nile region) during the medieval Christian period. Many are new discoveries and all are hitherto unrecorded.

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Radiocarbon dating, already well known to readers of ANTIQUITY (see Nos. 100, December 1951, and 101, March 1952) is quickly taking its place as a key to the chief problem of archaeology, which is to construct a system of absolute chronology. The dates given will gradually increase in value as they become more numerous; it will not be until *many* samples from different sites have been dated that we shall be able confidently to determine the range in time of a given culture. Already a few freakish results have appeared to warn us not to put too much trust in a single analysis; though the cause of these aberrations is unknown, there is no reason to attribute it to the method. The need for an accumulation of dates being recognized, the corresponding need of having them regularly presented in a form convenient for constant reference will be obvious. That need so far has not been met; publication lags behind performance. Archaeologists eagerly await each release, but generally first hear of the dates ascertained in a confidential letter or conversation. The published facts are not always easily accessible. Perhaps all this is to some extent inevitable at this early stage in the development of a new technique, and we hasten to add that our criticisms are made in no carping spirit but with a sincere desire to suggest a remedy. We archaeologists owe the discoverers an enormous debt of gratitude both for the revolutionary discovery itself and for the measures they have taken to use it for the advancement of archaeology. These measures must have involved much labour and expense which they, not we, have contributed.

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Here is an outstanding example of the dependence of workers in one field upon those in another. In such cases coordination by some body concerned with science as a whole would seem to be indicated. One thinks at once of such famous foundations as the Smithsonian and the Carnegie Institute, or of the various national museums. What working archaeologists need is the regular publication of a concise list of all the dates ascertained, with the usual brief facts of provenance. Such publication might be annual and take the form of a special Bulletin, which would be assured of a wide circulation, if launched with the necessary publicity. Archaeological museums and the libraries of archaeological societies would have to take it in, as it would be indispensable. The cost of printing or multigraphing would not be great, as the information would be concisely stated. The longer this problem remains unsolved the more difficult it will become, for results are accumulating rapidly.

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The word *glacis* is a technical term of military architecture, used to denote part of a circumvallation. It is a loan-word from French and originally denoted a ' place made slippery by frozen rain' (O.E.D.). By analogy it was applied to a sloping revetted surface beyond the *counterscarp*, on the outer side of the ditch surrounding a fortress. This was designed so as to be swept by fire from the ramparts, and was naturally made as smooth as possible—hence the name. Archaeologists, however, have used the word *glacis* to describe the revetted inner wall of the *ditch* which in military parlance is called the *escarp*. In many prehistoric defences the *escarp* slopes less steeply than in those of modern times, though always more steeply than a *glacis*. The intention of those who miscall an *escarp* a *glacis* is probably to indicate that the *escarp* was revetted—for many were not. But a *revetted escarp* is not a *glacis*; and if technical terms are borrowed they should be used correctly. In her recent article on Early Jericho (September, p. 118), Miss Kenyon acknowledges the incorrectness of her use of *glacis*, to which Colonel Sir Charles Arden-Close had called her attention; but she proposed to continue using it because the term had became so well established in Palestinian archaeology. Returning to the charge in a friendly letter Sir Charles expresses the hope that ' it is not too late to stop this incorrect use of a well-known technical term'. He refers to Humpty Dumpty who claimed that a word meant what he wanted it to mean; but we do not know whether his claim was generally admitted as an unfortunate accident ended his career. The blasts of Joshua's trumpets could not entirely destroy the walls of Jericho, and it seems doubtful whether those of Sir Charles and the Editor combined will shatter an established but erroneous terminology. However this may be, a great gulf (or *ditch*) is fixed between the *revetted escarp* and the *glacis* in profane lands. Perhaps after Christmas an agreed solution will emerge.

Archaeology and the Transmission of Ideas*

by SIR MORTIMER WHEELER

THIS lecture was written on shipboard in an untidy sea, and, if its matter and its sentiment pitch and toss a trifle, you may properly expect me, as an archaeologist, to blame environment. I belong in fact to a generation of students who have learned to make something of a fetish of environment, even at the expense of forgetting occasionally the extent to which we are the authors of that same environment. It is not difficult to think of examples where mankind has created a desert and called it civilization; and when in the long run the desert wins, as is its habit, we once again short-circuit logic and arraign environment.

Today I want if possible to subordinate environment as a dominant factor and to invite attention for a while to the transmission of ideas from place to place extra-environmentally, bridging the chasms between environment and environment, and providing that intellectual nexus which seems in our present-day affairs to elude us as often as we reach out for it.

In this attempt I would make it clear that I have no desire to revive the worn-out controversy which amused us for a while a generation ago. I refer of course to the ancient battle between the diffusionists and the separatists: between those who would derive similar ideas from a single common source and those who advocate the multiple and independent invention of such ideas. Today we are all essentially diffusionists, in moderation; we may say that the theory of diffusionism has proved its strength by surviving the speciousness of its own advocates. At the same time we need not exclude the possibility that two different men, living in two different places and ages and without cross-reference, may both have invented the safety-pin. Our modern philosophy is perhaps that of *Animal Farm*: diffusionism and separatism are of equal value, but diffusionism is the more equal of the two.

What I am concerned with today, however, is something rather different, if not altogether unrelated. As an archaeologist my métier is the description and discussion of material things: pots and pans and penknives, hearths and ashpits, the *disjecta membra* of the past. And from time to time, I must freely confess it, I am filled with doubt as to the uniform significance of this laborious task. I have quoted Polybius in this context,[1] and unblushingly do so again:—'Those who believe', says the wise Polybius, 'that they can obtain a proper general understanding of history from a study of its fragments are much in the position of those who might think that they could comprehend the life and beauty of a living thing from the *disjecta* of its once living body'.

Or again, as I would put it, the archaeologist will find the tub and will completely miss Diogenes. He will write profound papers on the Typology of Tubs; he will classify Tubs in categories A, B and C; he will discover a Tub-bearing Folk and plot their wanderings upon geophysical and phytographical maps. Provided that his views are not too revolutionary, he may aspire to become a Fellow of a Learned Society. *Only*, he has overlooked, and could not help overlooking, the one significant thing about that Tub, namely, that it sheltered the eminent Cynic and symbolized his philosophy for all time.

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* This is the substance of the Marett Memorial Lecture given at Exeter College, Oxford, on 7th June, 1952, and here printed in its lecture form.

[1] In ANTIQUITY XXIV (1950), 126, where I have discussed the question of values at greater length.

To put it differently, we must, I am afraid, confess it, that we archaeologists, most of us, are very little more than grammarians.

We are at any rate inordinately occupied with the *grammar* of our subjects. Our midnight oil flickers drearily over scraps of pottery, chips of stone, flotsam and jetsam of one kind and another, which we group in small crossword patterns and label 'cultures'. Now and then, but all too rarely, we remind ourselves of the fortuitous and incomplete character of these 'cultures'; for the most part, we are content in practice to regard them as entities and to move them round the board as chessmen in an eternal game of skill and chance.

And that that is a part of our job I should be the last to deny. Admittedly, sound grammar is a part of scholarship; but admittedly also, the codification of grammatical (or archaeological) minutiae is only a small part of scholarship, and my complaint is that, at the present time, archaeology has too few larger aims in view. Let us confess it: we are bedded down in a patch of stony soil, when some at least of us should be ranging over the wide horizons. Where today are our Schliemanns, our Flinders Petries, our Arthur Evanses, our John Marshalls? Men who, after their various fashions, added whole civilizations to our knowledge and on a broad impressionist canvas enlarged our comprehension of human achievement a hundredfold? Is our world grown so small, is there so little of magnitude left to do? Must we henceforth for ever foreswear the epic and content ourselves with syntax and gender-rhymes? Have we no young men with the imagination to raise archaeology once more to its place amongst the humanities? Or are we to degenerate into technicians, as a recent American writer would have it. Listen to him; for he fortifies himself with a bibliography of 612 authorities. He says 'Archaeology *per se* is no more than a method and a set of specialized techniques for the gathering of cultural information. The archaeologist, as archaeologist, is really nothing but a technician'. Do you believe that? I don't, for a single moment, but we must admit that our American friend has certainly scored a debating point.

For, let me repeat, Where today are our giants, our men of large understanding, of imagination? Their work awaits them, plain for all to see. In Asia alone are as good fish as ever came out of it. Examples leap to the mind. Away out on the steppe of Afghan Turkestan lies the great buried city, 7 miles in circumference, where Greek and Chinaman, Indian and Turkoman, met century after century to barter goods and ideas. Five hundred miles from it, on the arterial plain of Peshawar, towers another city which was already a metropolis before Alexander the Great received its submission in the year 326. Today its great mounds rise 100 feet above the plain into which they are slowly dissolving. Here are nodal points in the history of civilization: at present we know nothing of them. And there are countless others, awaiting the young man—let me say, if you will, the young Oxford man—with the imagination and the determination to add to substantive *knowledge*, not merely to accumulate and peddle *information*.

Our trouble is that we are, as it seems to me, becoming excessively insular in these matters. Politically, the world is shrinking upon us British folk and culturally we insufficiently resist the shrinkage. The work which in fact we do is on the whole pretty good within its limits; but its limits are pitifully too narrow. The great Haverfield did us for a time a sterling service in giving our classics a local application; Romano-British archaeology has reached an uncommonly high level of technical excellence; but it has also diverted into a relatively insignificant channel talent which might have been more amply deployed. Not long ago a young Cambridge graduate came to me for advice as one about to undertake the study of Roman Britain. My advice was that given once by Mr Punch in other circumstances: *Don't.* Instead, I bade him take a jeep, go to North Africa, stake out a strip of Libya 50 miles wide and in depth from the sea to the Sahara, and make a complete surface record of what he saw there. On the strength of his first reconnaissances he has climbed into a Trinity fellowship, and deservedly, for he is rapidly adding significant matter to our very imperfect knowledge of one of the richest provinces of the Roman Empire. That is a task well worth the doing. For a parallel problem on an even more productive scale I commend unreservedly the study of northern Iraq between the 1st and 6th centuries A.D. Sir Aurel Stein and others have shown us that upstanding remains abound there: behind them is a thin but useful historical narrative; and results can be guaranteed if the attack is steadfast. We may be glad, incidentally, to know that the Iraqis themselves are at last tackling one very relevant problem there, that romantic Arab stronghold, Hatra, away out in the desert between the Rivers, and tackling it (I understand) with more success than fell to the lot either of Trajan or of Septimius Severus, hard enough though they tried.

My first plea then is for the broadening of the basis of our archaeological research. Let us train here at home, where opportunity under close criticism is abundantly available. But let those of us at least who are busied with pre-medieval antiquity unashamedly cultivate that 'fatigue du nord' which, as Madame de Stäel observed, drove Winckelmann to his life's work in the south. Let us follow and overtake him in the lands of the sun where pre-medieval man of many eras found his fullest fruition, expressed himself most eloquently and had most to tell us about his reactions to life and his neighbours.

That appeal brings me to the second and main point of my remarks this evening. Even a generously equipped civilization such as I have just envisaged may conceal from us a very great deal of its essence. Social anthropologists are constantly lamenting the difficulties which they experience in extracting significant truths from the living subjects of their study. It is scarcely to be hoped that purely material debris will be more eloquent. Some ancient civilizations are of course more vivacious and informative than others. Some, on the other hand, have filled museums and still leave us guessing. Of one of the latter civilizations I would now say something, taking it merely as an illustration of principle, not as a substantive study.

In 1924 Sir John Marshall, then Director General of Archaeology in India, announced in the *Illustrated London News* the discovery of a new civilization. Its habitat was the great plain of the river Indus, which in its lower course flanks the south-eastern bulwarks of the Persian plateau. Today we can identify over 60 sites of that civilization, extending for 1000 miles from the Arabian Sea to the foot-hills of the Himalaya, an area twice that occupied by either of the partially contemporary riverine civilizations of Mesopotamia and Egypt. In date, the Indus civilization flourished round about 2000 B.C. We cannot affirm that it existed much before 2500 B.C. or much after 1500 B.C.

This great civilization was literate, but its language, like that of the Minoans or the Aztecs, cannot at present be read. Its 'hieroglyphs', if such we may call the elements of its notation, are unique. They represent a relatively mature pictographic script having no known roots or affinities; showing no kinship, for example, with the script of its senior neighbour, Mesopotamia.

This is number 1 point. Number 2 is this: the civic life of the great Indus capitals, Harappa and Mohenjo-daro, was, in its prime, of a highly developed order. At Mohenjo-daro, the better-known site, the town is rigidly and regularly laid out in oblong blocks or *insulae*, with broad main streets and a far more adequate drainage-system than any non-Westernized Asiatic town is likely to show today. I shall say more about these features presently. Rule was focused on a high-standing citadel, shared by a religious and secular hierarchy which was doubtless indivisible.

Two outstanding features, then, of the Indus civilization are a fairly evolved script and an evolved and disciplined civic life : two very notable landmarks in the progress of human society. Exactly the same features are of course, in general terms familiar to us in the Mesopotamian civilization, which may be nearly a millennium earlier in its adolescence. Now Ur of the Chaldees is 1400 miles, Susa 1200 miles from Mohenjo-daro, but the deltas of the Twin Rivers and the Indus are connected by an easily navigated Gulf and by a coastal belt which, in the desiccated condition of a later age, was still traversable, however reluctantly, by Alexander's tired army. If, then, we ask, 'Whence, in the middle of the 3rd millennium B.C., did the Indus folk derive those two essentials of civilization, writing and city-life?' the answer would appear to be an easy one: surely from the mature cities of neighbouring Mesopotamia. But wait a minute.

The problem is not so simple as the bare statement of principle might imply. When we look beyond general principle to detail, it is the *differences* between the two civilizations that impress us at every turn. I shall mention some of these differences a little later; meanwhile it will suffice to say that a citizen of Ur in Mesopotamia would have found himself in a substantially alien world, had he been transported to a street of Mohenjo-daro in the Indus valley. Ideas even in so portable a form as improved tools and weapons failed to reach the conservative Indus citizens, who obstinately clung to rudimentary types long outmoded in the West and North-west. And once more let me emphasize the complete dissimilarity between the two scripts, whose only community lies in the *basic idea of writing*.

Basic ideas : can we postulate a traffic in *basic ideas* devoid of all material inter-resemblance? At this stage archaeology threatens perilously to encroach upon philosophy, and I for one rejoice in the peril. In an age when materialism threatens on all hands to clog our thinking, and not least our archaeological thinking, I should like to believe for a moment that there is an archaeology of ideas, an archaeology that we cannot imprison in a museum-case, an archaeology that is something more than a superior sort of philately. In stating that aspiration, however, I am reversing the actual process of thought. Let me explain.

I was standing one hot summer's morning amidst the upstanding remains of a remarkable palace near Agra,[2] in the vast plains of northern India. The palace had been built by the third and grandest of the Grand Moghuls, Akbar the Great, in the time of our Queen Elizabeth I, on a patch of desert chosen for the fortuitous reason that Akbar's holy tutor had lived hereabouts. Unfortunately for the project, its designer had failed to consider that the amount of water required by a Muslim saint was disproportionately less than that required by a capital city, and within a single generation the new capital was the dead, desiccated, jackal-haunted monument that it is today. Early in the 17th century an English visitor could already describe it as 'ruinate, lying like a waste district and very dangerous to pass through at night'. To wander through its empty halls is today an experience not lightly forgotten. They have all the features apt to Muslim religious, administrative and domestic life on the grand scale : the diwans of public and private audience, the treasury, the stables, the hammams, the harems, the pavilions and observatories, the towering cathedral-mosque, the floor-ring to which (it is said) was tethered the rogue elephant retained for summary executions, the variegated chequerboard pavement upon which (again, it is said) dancing-girls stepped out the game of backgammon with the deliberation of a slow-motion ballet, the high gate where the imperial fifes and drums noisily heralded arrival or departure, the plashing water laboriously elevated by a vast machine to this barren spot. But amidst these kaleidoscopic memories, one impression remains dominant : that of the alien, non-Islamic idiom in which the conventional Islamic concepts are expressed.

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[2] At Fathpur Sikri. See E. W. Smith, *The Moghul Architecture of Fathpur Sikri* (Allahabad, 1894), and M. A. Husain, *A Guide to Fatehpur Sikri* (Delhi, 1937).

To carry this impression to those who have not stood in the footsteps of Akbar demands a brief reminder of certain basic facts, for which I must beseech your momentary patience. I need scarcely recall to you that Muslim architecture, with the Prophet himself, was born of the desert. It was nurtured amidst broad, bare horizons, broken if at all by the hard geometry of rocky ranges. In conformity with this setting, its most distinctive architectural expression is a simple cube or group of cubes, with rigid outlines interrupted only by a sudden mountainous dome or a spiky minaret. Within all this rigidity the surface may be variegated by tilery or painting or flat carving, but the geometrical framework remains the dominant note.

Now when invasion carried this austere tradition to the Indian sub-continent, it would have been difficult to find a more completely alien landscape into which to transplant it. Medieval India was first and foremost a land of jungle and of close horizons. Today, with a quadrupled population and organized development, it is difficult to realize the former extent of 'the trackless, pathless, boundless forests' in which Rāma and Sita and the Pandavas of Indian epic moved and had their being. The medieval architecture of Hindustan is fraught with the influence of this jungle setting. Swarming, monstrous, sinister, fantastically beautiful, its tangled background is reflected in the often monstrous and sinister sculpture which envelopes the tortured surfaces of the Hindu temple ; in the guarded depths of which, in the no less mysterious darkness of a small flower-decked cell, lurks the ultimate godhead in the pale flicker of an oil-lamp. Nothing could stand in greater contrast to this luxuriant and recondite jungle art and architecture than the solemn sobriety and open airiness of the architecture of the desert. In the whole history of building, there is no more fascinating episode than that of the reconciliation of these two traditions—as in India reconciled they were.

One example of this reconciliation will suffice, though many could be given. As we tread the vacant courts of the derelict Moghul capital to which I have invited you near Agra, we come to the little building, still intact, where the sultan received his favoured visitors in private audience. This is normally in the Islamic world an arcaded tent-like pavilion with a raised floor whereon the becushioned host reclined, overlooking a garden to which his guests were admitted. Not so is the remarkable structure which we are now approaching. It rises two stages high, girt with carved verandah and cornice and crowned by airy pavilions. Within, in the midst of the tall reception-chamber, rises a carved column, branching weirdly tree-like into thirty-two knarled and twisted brackets upon which at a height of 14 feet above the floor, rests the exalted throne-platform behind lace-work balustrades. Thence to the four corners of the building air-borne passages offer access to the balcony which snake-like brackets carry round the room. Here and there the discerning eye may identify carved enrichment from the far-off western highlands, but the whole conception is that of a petrified jungle growth, a fantasy of Hindustan as alien to the traditions of Islam as are the tropics to the stony plateau (*frontispiece*).

Truly there is no building like this in the home of Islam : Persia, all Arabia, cannot show the image of it. Functionally it represents a normal feature of the palace plan and few but good Muslims used it. In other terms, the *idea* of it was an integral convention of the intrusive Moghul administration. But architecturally and artistically it is, as I say, a native Hindu extravaganza, and nothing else. So also in varying degrees are the other buildings which frame its horizon : the piled pavilions of the observatory, the massively carved verandahs of the harems, even the fretted outline of the great mosque itself. The *idea* of mosque and to some extent that of pavilion came with the invader from the plateau and the desert. Nevertheless, here the *expression* of the idea is Indian again and again. The austerity of the desert has on all sides been tempered and clothed by the diversity of the jungle ; so much so that we are constantly faced with the fact that the idea itself has become little more than an abstraction ; transmitted, as history tells us, but materially almost utterly transformed.

Here, in an example closely controlled by written record, we have, I venture to think, something of considerable importance to us when we come to analyse cultural inter-relationships beyond the reach of writing. We archaeologists are, no doubt properly but, I suggest, sometimes excessively, bound by material evidence. Had our Moghul palace with its dominant Hindu aspect been four millennia instead of merely four centuries old I know not what theses we should have boiled out of it ; scarcely, I suspect, the truth, that, in spite of its overwhelmingly native quality, it represented the most successful and far-reaching alien domination of India in pre-British times. We may sometimes forget—perhaps we are wise in forgetting—that ideas have wings and are liable to flit airily about our prehistoric as about our historic landscape, with a baffling intangibility or disguise. Governed by that thought I am now going to return to the problem of the prehistoric Indus Civilization of which I made some mention at the outset.

At the risk of weariness, let me analyse a little further the points of similarity and contrast between the Indus Civilization and its older contemporary in Mesopotamia. Both represent an evolved city-life expressed in cities up to 3 or 4 miles in circumference. Both, at the end of the 3rd millennium, exhibited town-planning, although in Mesopotamia Ur incorporates from its early beginnings a rambling village lane or two, much as the grid-plan of New York fits uneasily on to the rural vagaries of its pre-civic Broadway. This variant feature suggests that at Ur (as at New York) we have the whole process of evolution from village to city, whereas in the Indus valley the civic idea was already mature when Mohenjo-daro took shape. Architecturally, the Mesopotamian cities are largely of mud brick whereas the Indus cities are typically of baked brick, but that is presumably a matter of climate. Connected with this factor is the occasional use of the voussoir-arch in Mesopotamia as against the invariable use of the corbel-arch in the Indus region. And the combination of baked bricks with a damp climate stimulated the Indus folk to build drains to an extent unparalleled elsewhere in the ancient East. Drains everywhere. The houses and streets of Mohenjo-daro are packed with drains, scrupulously equipped with manholes for their better clearance. Nor did the idea of sanitation end there: in the Mohenjo-daro houses, upstairs or down, are little garderobes which hold an honourable priority in the history of domestic convenience—a priority scarcely challenged by the rare occurrence of a similar contrivance in Mesopotamia. But when we consider the generalities of architectural design the contrast between the two civilizations is striking. The seeming simplicity, amounting to parsimony, of detail in the Indus buildings is no counterpart to the elaborate panelling and variegated colouring of the more monumental structures of Sumer. Whether the Indus folk, like their successors in the sub-continent, compensated by the use of carved timber, we shall never know. It may be that they imported stone dressings to a rather larger extent than has been realized ; I cannot forget that in my last cuttings at Mohenjo-daro in 1950 I found ribbed marble and polished limestone blocks prepared for use on a timber framework which had apparently included columns. Nevertheless, the unquestionable fact remains that the architectural aspect of the two regions had little or nothing in common. There is far more resemblance between a Mesopotamian ziggurat of the 25th century B.C. and a Mexican temple-pyramid of the 15th century A.D. than between the former and the contemporary citadels and monuments of the Indus valley.

So too with the arts and crafts. I need not go through them categorically, but to emphasize at the same time their separateness and their parallelism in the two civilizations I may cite one or two of them. The representational art of the two regions we may dismiss with the remark that there is no apparent nexus whatsoever between them. Of more interest is the extensive use of sealstones in both regions, and here, as in matters of town-planning and writing and indeed in the general principle of civilization, there is a potential link for consideration. But the famous square seals of the Indus valley with their vivid representations of animals—life-like bulls and elephants, rhinoceros, tiger, crocodiles—have nothing to do, save in the abstract *idea* of sealing, with the seals of Mesopotamia. And of the isolation of the Indus script, preserved to us on seals and potsherds, I have already spoken. The abstract *idea* of writing is here again the only connection with the outside world. Occasional traffic between the two river systems, whether by sea or land, as represented by a few Indus objects in Mesopotamia or Mesopotamian objects in the Indus valley, is not now at issue. Even the occurrence on a few of the Indus seals of a figure resembling the familiar Gilgamesh of Sumer—a Gilgamesh, be it noted, here equipped appropriately with tigers in place of the traditional Mesopotamian lions—may imply a common source rather than an inter-borrowing. All in all, save for a sharing of certain abstract *ideas*, the two civilizations stand strangely apart from each other.

At this point a word is again necessary about relative chronology. I have said that in origin the Mesopotamian civilization is the older of the two. More precisely, we may say that, in Mesopotamia, civilization in the full sense of the term was taking shape about the middle of the 4th millennium. By 3000 B.C., and perhaps considerably earlier, it was mature. On the other hand, let me repeat that recent re-assessments of Indus chronology by Professor Stuart Piggott and myself have suggested a date about 2500 B.C. for the early maturity of the Indus civilization, and in the light of the deflationary trend of protohistoric chronology in Asia I doubt whether it will now be pushed back to an appreciably higher period. In other words, the Indus cities are, by a comfortable margin, the younger contemporaries of those of Sumer and Akkad, and, in so far as there was an interchange of basic ideas, must be regarded as the receivers rather than as the donors.

Let us now sum up. At a time when civilization was a new phenomenon, the idea materialized on a large scale in succession on the two flanks of the Persian highland. Within the complex conception of city-life, the two civilizations shared in varying degrees such fundamental and revolutionary ideas as town-planning and writing. Some integral community of inspiration in these matters is difficult—I would say, impossible—not to postulate. So different, however, are the forms of expression within the two parallel civilizations that anything approaching a literal colonization of the one from the other is unthinkable. What then are the mechanics of the business ? There is no mystery about the physical transmission of our own alphabet, for instance, from Greece to Italy ; but there is every sort of mystery in the thought of the transmission of a *disembodied idea* of writing from the Euphrates to the Indus. What is the answer ?

If the idea *was* in fact disembodied in any literal sense of the term, we are left with the obvious and difficult question : What then did the one region in fact receive from the other ? How is it possible to conceive an *idea* of writing save as an *embodied* script ?

Or an *idea* of civilization save in a *builded* Babylon ? Seals and such fry are another and easier matter. Pendants which could be used as seals were already current in northern Mesopotamia as early as the 5th millennium B.C., when civilization was still a distant cloud no bigger than a man's hand; and multiplying lines of descent would be expected to produce the infinite local varieties of seal which in fact we recognize in the Middle East a millennium or two later. So also with tools and weapons of metal. Such things are of no great import; they do not predicate an evolved society. I have here merely cited their divergence, as between the Indus Valley and Mesopotamia, in order to stress the absence of any significant cultural pooling between the two civilizations. Neither common ancestry nor casual interchange can be conjured up, it would seem, to unite the diversities of the two scripts or of the two civilizations as social phenomena. No supposititious cousinship can be adduced as the easy answer. For Civilization and Writing are the sudden and substantive response to new opportunities and decisive challenges, not, like seals or socketed axes, the incidents of fashion and provision.

Admittedly at any level the colonization of ideas or modes is a strange and complex process. Their power of adaptation would appear to be almost infinite. The recipient or patron alone may exercise an astonishing control over the shape of the skills and notions which he is pleased to patronize. Persian carpets, for example, were needed in America; nevertheless, the makers of the beautiful traditional rugs of Kerman—the aristocracy of their craft—found a poor market in New York until, by a process of systematic enquiry, they discovered that Louis Quinze patterns were the mode there. So they immediately switched over to pseudo-Louis Quinze, and today in the whole of Kerman you will not find a modern Persian rug worth looking at. In New Mexico, the pueblo Indians have never ceased to make magnificent pottery of neolithic or chalcolithic type, with dignified, restrained decoration. But they are Barbarians, and, as every good American patron knows, Barbarians love bright, garish colours. So these poor Indians are now perforce buying up bright exotic reds and blues and yellows, and subordinating their own aesthetic consciousness to the misbegotten concepts of their overlords. My only point here is, that in fashions such as these a skill may be broadcast, as it were, with an altogether *alien* cultural content.

On partial analogy it might, I suppose, be averred that the germ of writing was transplanted from Mesopotamia to India and was there reshaped in some subtle manner by the *genius loci*, to emerge as the Indus script, without detectable transition and with no visual trace of cultural parentage. I do not know. I am not quite happy about it. That absence of material transition from old idea to new form is a little baffling. I am inclined, however, to think that our Islamic parable, our Agra palace of the 16th century A.D., brings us as near to a solution as we can attain. In other words, let us admit that, by a process unknown to natural science but on occasion fully documented by the historian, ideas can and do simultaneously extend their habitat and radically change their specific form. It takes a million years to develop a natural species; perhaps a million seconds to develop an idea. We archaeologists must be prepared to dematerialize our thinking when we consider this vital problem of the diffusion of ideas. The prospect is not a very happy one for us to contemplate. There is a completeness, a satisfaction, about the easy, objective evolution of pot-handles and safety-pins. Intangible things such as ideas are dangerous and treacherous phenomena, and, to an essentially material science, not very respectable. I am afraid that my offer to the archaeologist smacks perilously of blood and tears.

I turn now to another aspect of the transmission of ideas, again using the Indus Civilization as my example. It may be presumed that almost every major effort of the human intelligence contributes something, however indirectly, to the aggregate of human achievement. It is comforting, however, when as archaeologists or historians we can actually trace the causative sequence and affirm quite precisely that A gave rise to B or that D owed so-and-so to C. In this sense our modern debt to the ancient civilization of *Mesopotamia* is not in doubt; to illustrate it by small but vivid examples we need merely remind ourselves that such significant commonplaces as the 360-degree circle and the 60-minute hour are an inheritance from that ancient land. But what does the later world owe to the sister-civilization of the Indus? I ask because we know (or think we know) that in the 2nd millennium B.C. the lands of the Indus were overrun by mobile barbarian invaders, whose triumphant hymnal, the Rigveda, has survived to us as the earliest Indian literature. I ask also because we can say that the best known of the Indus cities, Mohenjo-daro, ceased suddenly to exist, as it were on a Wednesday afternoon at 4 o'clock, never to rise again. Did all that it stood for perish suddenly with it in that hour? Can we indeed lightly dismiss this civilization as a 'false start' in the story of human endeavour?

I have no doubt that the Indus civilization failed in fact long before its actual end; that the sudden catastrophe to which I have just referred was actually in long preparation. But that is another matter: it is a familiar axiom to economic historians that an age of fulfilment, such as is implied by a great civilization, is accompanied by an increase of population which, after a phase of balance, outstrips food-supply and leads to a decline in standards of living. That is of course happening to us today. It happened nearly 4000 years ago in the Indus valley, and as archaeologists we can still detect symptoms of the process. In the later phases of the city, the houses of Mohenjo-daro are everywhere subdivided into tenements by relatively shoddy party-walls; streets are encroached upon; squalor prevails, the squalor of a swarming and increasingly low-grade population, living in a city but with fewer and fewer of the amenities and disciplines of civilization. To such a society, the end may come easily and swiftly, by disease or violence. At Mohenjo-daro, the picture is clear enough. Let me recall it briefly; for the suddenness and completeness of the disaster sharpen its significance in the present context.

On the highest and latest level of Mohenjo-daro, after the city had been in occupation for perhaps a millennium, the excavators found at several points the evidence of sudden tragedy. Women going to a public well were struck down and their bodies left lying across the steps and in the adjacent street. In another lane lay six skeletons, including a child. Elsewhere, nine skeletons including those of five children were found crowded together in 'contorted attitudes'. Elsewhere again, in the room of a house a dozen men and women and a child were found together in varied positions indicating simultaneous death. And there were others. It has been suggested that a plague was responsible for these sudden deaths, but a plague does not leave the deep gashes which at least two of the skulls exhibit. There can be no doubt that these poor bones represent the victims of a massacre, the handiwork of men who swept suddenly over the city and then passed on. Even in the East one does not live happily or long in a city with the unburied dead lying about in the streets, and it is evident that the slayers did not linger.

On other occasions I and others have given some reason for connecting this episode with the accounts in the Rigveda hymnal of Aryan assaults, somewhere about the middle of the second millennium B.C., on the cities of the Indus system: when the Aryan war-god, Indra, aflame with the fiery drink of the gods, stormed through the countryside, slaying the dusky natives by the 'hundred thousand' and rending fortresses 'as age consumes a garment'. It may indeed be that the attack on that other great Indus city, Harappa, in the Punjab some 400 miles from Mohenjo-daro, is explicitly mentioned in the Hymns. There archaeology is at fault owing to the systematic destruction of the upper layers of the site by railway-engineers in the 19th century. In the light of Mohenjo-daro, however, the general picture presented by the Rigveda is clear enough. Materially, the Indus civilization was wiped out; archaeologically, the episode was closed.

But again wait a moment. Before we dismiss the Indus citizens as a mere archaeological curiosity, there are other points for consideration. The destroyers were uncivilized folk, ignorant of the art of living in cities. We may imagine them, like the barbarian German tribesmen of a later age in Gaul, shrinking in awe from the ghost-ridden ruins of the cities which they had sacked, regarding them as 'graves surrounded with nets' (I am borrowing the words of Ammianus Marcellinus). And yet, the awesomeness of these newly dead Indus cities must have brought something of its own revenge. Their enslaved survivors, for ever outcast as Dasyus or Dark Folk from the ranks of their fair-skinned Aryan conquerors, must nevertheless have retained the prestige of intimacy with the endemic gods and the traditions of the land, and have passed on something of their secret knowledge as they tended the children of the newcomers. It can be no chance that on certain of the Indus Valley seals we find a squatting deity, horned, tri-faced, ithyphallic, grim, whom Sir John Marshall was doubtless correct in recognizing as the prototype of the sinister Śiva of medieval and modern India: the all-powerful god who has nevertheless no place in the theogony of the Aryan invaders. On one of the seals we see him surrounded by animals—tiger, elephant, rhinoceros, bull, ibex—in the character, we may suppose, of Paśupati, Lord of Beasts. Less certainly, we may identify him in the torso of a sculptured dancing figure, as the familiar Natarāja of countless South Indian bronzes of a more modern age. Again, the phallus-worship which is an integral feature of historic Hinduism has no root in the Aryan religion of the Rigveda but appears to have flourished in the Indus cities. And yet again, the dominance of the bull in the Indus Valley is beyond dispute: for example, of a hundred terracotta figurines which I found at Mohenjo-daro in 1950, no fewer than ninety represented bulls. The sanctity of the bull has made this animal a notorious economic problem of modern India; but the Aryan invaders had no compunction in regarding it as a normal article of diet. The animal's present embarrassing immunity in India may well be an inheritance from the Indus folk rather than from their Aryan successors.

I claim no originality for this and similar analyses and need not here carry the matter further. Suffice it to say in summary that religious ideas of high importance in later, so-called Aryan, India are certainly or probably legacies from the pre-Aryan Indus Civilization, and represent a continuity on the religious or intellectual plane where any material nexus is lacking. Whilst therefore we cannot affirm, in anything approaching the classical sense, that the captured Indus folk captured their rough conquerors, we can at least proclaim a continuity of important and even controlling *ideas* through the period of material destruction and revolution. Tiny but significant particles of evidence hint that, whilst the Indus urban system, its street-plans, its endless drains and its astonishing seals, vanished into thin air leaving not a wrack behind, something of the Indus thought and its way of life live on into modern, 20th century India. Whether we should add that its unread semi-pictographic script is parental to the Brahmi script of historic times is matter of dispute into which I am not qualified to enter, and no claim is made for it. In any event, the main vehicle of thought in ancient India was an impeccable oral tradition, and the limited use and seeming fossilization of the Indus script was doubtless due in great measure to that fact.

Lastly, I turn through fifteen centuries to one more example of the transmission of ideas, an example of a different kind. This time we are in the first five centuries A.D.

and on the northern fringe of the region with which we have just been dealing. A few months ago, the Press of another university published a monumental work in three volumes on the subject of *Taxila* by my distinguished precursor in India, Sir John Marshall. Taxila is today an obscure railway-junction on the borders of the Punjab and the N.W. Frontier Province of Pakistan: not quite so obscure as 'Babylon Halt' on the railway south of Baghdad, but of that order. Anciently for a thousand years, from about 500 B.C. to A.D. 500, it was a capital city successively under Persian, Indian, Greek, Scythian, Parthian and Kushana rule. It lay on the great highway which tapped the trans-Asian 'Silk Route' at Bactra near the Oxus and, penetrating the Hindu Kush, carried an international trade from and into India and the lands beyond.

With the varied history of this site, as recovered by excavations carried out for nearly a quarter of a century on a lavish scale, we are not here concerned. But one aspect of its later years illustrates and emphasizes a well-known problem in the history of Asiatic art, and I would cite it as a strange example of the effective penetration of ideas and formulae over many thousands of miles of country and through countless barriers of nationality.

The story is briefly this. By way of preface, let me recall that, for more than a century and a half after the death of Alexander the Great, a Greek aristocracy dominated the Oxus valley, in Turkestan, from that great Mother of Cities which I have already mentioned, Bactra, now the insignificant village of Balkh. Bactra, as I have said, stood at the junction of the trans-Asian 'Silk Route' and an important branch-line extending south-eastwards into India—the branch-line upon which Taxila itself stood, some 500 miles away through the high mountains of the Hindu Kush. Soon after 200 B.C. these Greeks of Bactria pressed southwards, evidently using the trade-route as their axis of advance, and established themselves at Taxila. There they ruled for about a hundred years; but early in the 1st century B.C. they were superseded here and elsewhere by Sakas or Scythians, who had come out of Central Asia, had overrun Bactria, and now swarmed over this ultimate province of the sub-Greek world. There is no evidence that anything significantly Greek hereabouts survived the onslaught.

I mention this early semi-Greek phase in our Orient merely to dismiss it from the present context. A subsequent cultural episode has sometimes been speciously fathered upon it. That subsequent episode is the sudden appearance in this region, in and after the 2nd century A.D.—perhaps as early as the latter half of the 1st century—of Western goods and Western modes and their partial absorption into Eastern culture It is possible that the earlier Hellenistic phase had in some remote way prepared the local consciousness for this new access of Greek, or rather Greco-Roman, ideas, but a local organic continuity between the two phases cannot be postulated. The hiatus of a century and a half (or more) which separates them cannot be thus lightheartedly bridged. Some new factor must have intervened.

Reference to ANTIQUITY XXIII (1949), 4 ff., will suffice to recall the general nature of this remarkable phase of Westernizing art, in which satyrs, putti, Antinous, and even the Trojan horse were integrated with oriental forms in the Buddhist art of a considerable tract of Asia. It has long been agreed that this is a very striking phenomenon, but the mechanics of it are not yet wholly explained. How in fact did this remarkable impact of West upon East occur? Well, we have, I think, a hint of the *modus operandi* in the *Periplus of the Erythraean Sea*—that famous merchant's handbook, compiled probably at Alexandria between A.D. 60 and 110 as a guide for the oriental merchant. From it we may infer that the rich luxury trade between the Orient and Italy under the early Roman Empire was, more often than not, diverted by Parthian hostility into a devious route which took it precisely through the regions with which we are now dealing

Instead of proceeding along the direct line through Parthia across the Persian plateau between the Mediterranean and Bactra, trans-Asian commerce was compelled by Parthian intransigence to turn aside at Bactra and to make a long detour southwards to the ports of Pakistan and N.W. India. Thus for two centuries, between A.D. 50 and 250, Afghanistan and what is now West Pakistan were busy with the transit trade of the Orient, and took toll (we may assume) of the commodities that passed their way. We are told by our Greek geographer that by this route from the Western world came figured linens, topaz, coral, frankincense, glass vessels, gold and silver plate, and wine. In return, from the East came turquoise, lapis lazuli, 'Seric' skins from High Asia, cotton cloth, silk yarn from China, and indigo.

Now let me emphasize that, so far as N.W. India and Afghanistan are concerned, this trade was, as I have called it, a *transit* trade. There was nothing, or almost nothing, native to those regions that the Western world required. They represented just so much mileage *en route* to the further East. But it is not to be supposed that princes and satraps along this route allowed wealthy caravans to pass their doors with complete immunity. Great towns such as Taxila, or Begram north of Kabul under the towering Hindu Kush, owed their prosperity and even their existence to the arterial highway that passed their gates. And at Begram we have an outstanding illustration of the fashion in which they collected their dues.

It is now well known—although publication is still very incomplete—that in 1937 and 1939 French excavators at Begram, digging in the ancient town there, found a building in which were two rooms with one or more blocked-up doorways. Packed in serried masses into these rooms was an astonishing array of wares. Carved ivories in great quantity and of exquisite quality from India and lacquer-ware from China lay side by side with bronzes and glassware and stucco medallions or *emblemata* from the Mediterranean world, from Alexandria and perhaps from Syria. The Roman glassware dates from the 1st, 2nd and 3rd centuries A.D., and it is evident that the collection represents an accumulation, not a single consignment. Its terminal date may be about A.D. 250.

Here, stowed away like so much bullion in a bank strong-room, we may recognize a royal depot of goods gathered as dues from the rich transit-trade of which I have spoken. China, India, the Roman Empire have all contributed to it. In the museum at Kabul, where most of the collection is stored, or in the Musée Guimet at Paris whither a part of it has been brought, the imagination of the spectator is stirred in the contemplation alike of its excellence and of its vast horizons.

In the present context I would draw a special attention to the plaster *emblemata* which I have just mentioned. They doubtless originated in Alexandria, and we are reminded how Rostovtzeff long ago affirmed that 'the active agents in the exchange of goods between the Roman Empire and China were the Alexandrian merchants. Without them the commerce with India would probably not have existed'. Here it is relevant to emphasize that Alexandria was also the Western focus of plaster or stucco sculpture, drawing upon those endless local supplies of gypsum which still whiten the coastline for miles to the west of the city. Without elaborating the matter at this late hour, we may assert that a convergence of evidence indicates an Alexandrian origin *in primis* for the Westernizing stucco sculpture which we have found amidst the Buddhist art of Asia in and after the 2nd century A.D.

But to discover the source of an idea, and even the mechanism of its diffusion, does not suffice. Ideas are sensitive plants and often enough perish in transplantation. For example, an active and well-documented Roman trade with Southern India, of which I have not spoken this evening, left *no* enduring imprint whatsoever upon the South

Indian cultures with which it came into contact.[3] The rich Western glassware of the Begram hoard itself had likewise no influence whatsoever upon the craftsmanship of India or Afghanistan. Why did sculpture, and particularly stucco sculpture, from the West enter as it did into the aesthetic consciousness of those lands, so that modes derived from it can be traced ultimately as far as China on the one hand and south-eastern India on the other ?

The answer is not in doubt. When this trade was at its height, Buddhism was the flourishing and fashionable observance in the lands of passage whereof we have been speaking. The Buddhist monasteries were wealthy, and ready to expend their wealth as patrons of craftsmanship. Above all, their faith was undergoing a radical change which at this moment made them peculiarly receptive to certain categories of western ideas. I need not do more than remind you of the circumstance. In origin, Buddhism was a way of life, not a religion. The Buddha was a teacher or prophet, and for some five or six centuries the representation of him in art was as rigorously forbidden as was that of the subsequent Prophet of Islam. But, by a process of evolution natural to a country where reverence for the teacher is (very properly) deep-seated, the Buddha gradually assumed the stature of a god to whom prayer was apt, and this new status had secured a formal recognition by the 2nd century A.D. By now the divine Buddha had begun to appear in person and had become the representational focus in Buddhist art. Both aesthetically and iconographically, the moment was ripe for revolutionary change.

For the emergence of the Buddha as a divine figure, with all that this implied in the coordination and centralization of artistic composition, at once created a need which was essentially novel in the East but was traditional in the classical art of the West. The circumstance that at this moment objets-d'art from the West were travelling commercially through the Buddhist zone of N.W. India and the adjacent lands was a fortuitous but a happy one. The need and the practical guidance as to its fulfilment thus coincided ; with the additional and simultaneous advantage that, incidentally, a new medium, stucco, was introduced from the commercial base at Alexandria and at once indicated an easy vehicle for the mass-produced reproduction of which the swarming hive of Buddhist shrines and monasteries was now in need. From now onwards, far along the Asian trade-routes which bore Buddhist monachism as well as more secular commodities, the new medium and elements of the Western art which had initially accompanied it found their way into the heart of Asia and as far as the borders of China.

Enough of this complex story. I have recounted a sufficiency of it to point a moral, which is this. Whilst on occasion (as we have seen in the Indus Valley) ideas may flit, incorporeally but nonetheless effectively, from zone to zone and age to age, on other occasions (I have especially quoted the case of South India) ideas in the most corporeal and archaeologically gratifying form—pottery, glass, coined money—may migrate and perish save as mere indices of effort. Only in some rare moment may the seed fall on fertile soil and fructify ; and to illustrate this I have given the example of 2nd century Buddhism with its new needs and coincidental opportunities.

My key-examples in this discussion have been taken from history or protohistory where the archaeologist is ultimately subject to literary control. But, poor fellow, such control is at the best available only for one hundredth of the time-span with which he deals. Pity him, I beg of you. Pity him groping in that other ninety-nine hundredths. He is a surgeon working in darkness and with blunt tools upon that most sensitive of organisms, the human brain. Happily his patient is already dead.

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[3] Unless we except the momentary transfer of Arretine rouletting to a certain class of native ware.

Cerdic's Landing-place

by O. G. S. CRAWFORD

IN 1931 I wrote an article[1] maintaining the thesis that Cerdic's ora, where in 495 Cerdic and in 514 the West Saxons landed, was to be located near Totton at the head of Southampton Water; and that he advanced westwards along an old track called the Cloven Way to Cerdic's ford, which is certainly to be put at Charford on the Salisbury Avon. I still think that the latter part of the advance must have been along the Cloven Way which led to the ford down a deep hollow way that can still be seen. But certain new discoveries have caused me to abandon the theory of a landing at Totton and to locate Cerdic's ora on the coast between Calshot and the mouth of the Beaulieu River.

A weak point in the Totton theory was the long interval of thirteen years between the landing and the defeat of Natan Leod, whose territory lay between Totton and the forest plateaux to the west and southwest. Those years were presumably occupied in consolidation and settlement, which could hardly have taken place within a mile or two of an undefeated hostile ruler. If, however, the landing was on the shores of the Solent ten miles to the southeast, there would have been plenty of room for settlement on the shores of Southampton Water and the Solent.

Now therefore I suggest that Cerdic's ora was the stretch of shingle-beach between Calshot and Lepe at the mouth of the Beaulieu River. That would exactly fit the special meaning of O.E. *ora*, a ' firm fore-shore or gravelly landing-place ' (Ekwall, citing Itchenor and Rowner) as opposed to places where the coast was soft and muddy. The word does in fact occur in the name Calshot itself (O.E. *celces ora*) and at Needs Oar, though as I have already pointed out there can be no etymological connection with Cerdic. The suffix alone survives in Ower close by. At the southern end of this beach is the farm of Stone (formerly a small medieval manor) which Ekwall identifies with Bede's Ad Lapidem. That identification is strongly supported by the fact that at Stone the Roman road to the Isle of Wight ferry reached the Solent. Bede's narrative shows that the ferry was still in use in the 7th century, and it survived down to the 16th.[2] The Roman settlement has yielded no positive evidence unless the old foundations and lumps of limestone to be found near Stone are remains of it.[3] It must have been at the end of the Roman road on the narrow tongue of land, about a quarter of a mile wide, between the Dark Water on the west and a nameless stream on the east. Before sluices were made in the shingle-bars at their mouths, both these streams must have had small tidal estuaries or ' lakes '; and before they were blocked by the shingle these would have been navigable, providing excellent havens for small ships. The mysterious earthworks in Pits Copse, south of Stone Farm (FIG. 1), look in plan like ships' docks, and whatever their age or purpose seem to belong to a period when the approach was still open and ships could sail in. If these were made in Roman times Cerdic might have used them, but he would certainly never have constructed them. One would not dare to claim more without definite evidence. For the present argument it is enough to accept the termination here of the Roman road as evidence that some sort of haven probably existed. I think we may credit Cerdic's 'intelligence department' with knowing about this, especially if he and his band came, as I believe, from Kent and not direct from the distant continental homeland. The Stone peninsula would be easy to defend, being surrounded on three sides by water ; and even without any previous knowledge it would be the obvious landing-place, confronting him as he sailed up Spithead. (The coast between Calshot and Stone Point runs in a direction S.W. by S., confronting Spithead).

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[1] ANTIQUITY V, 441–58 : ' Cerdic and the Cloven Way '.

[2] The two brothers of Arwald, king of Wight, fled from the island, ' et in proximam Jutorum provinciam translati ; ubi, cum delati in locum qui vocatur Ad Lapidem . . . ' Bede, Op. Hist., ed. Plummer, 1916, i, 237. In 1590 John Cole, a mariner of Lepe, found a mast floating in the Solent while he was ' in his boate passinge betwene Gurnard and Leape '. *Book of Examinations* (So'ton Record Soc., 1914), 78.

[3] See *Field Archaeology*, by Dr Williams-Freeman, 1915, 214–16, 442 : *Proc. Hants Field Club* x, 1927, 35–9 (plan of road and of earthworks in Pits Copse). The present farmhouse of stone is partly built of Isle of Wight limestone, and is of medieval date. There are banks and hollows in the west bank of the stream just north of the point where it is crossed by the private road leading east to the farm.

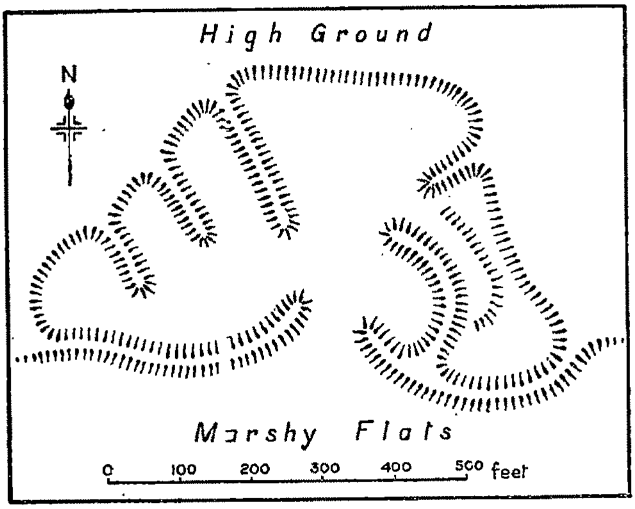

FIG. 1. EARTHWORKS IN PITS COPSE, STONE, HANTS
after Sanders in *Proc. Hants F.C.*, x, 1927, 39

From the Solent at Stone the Roman road runs northwestwards for five miles to Dibden Purlieu, beyond which its course is doubtful. Either it continued along the same alignment through Orestone Copse near Purlieu House, where I once thought I saw remains of the causeway ; or else it changed direction following the line of the Forest boundary, which is known to have followed an ancient road. Across both these courses runs a linear earthwark 1600 feet in length (number 1 on the map, FIG. 2), facing northwards (ditch on north). This is one of the new discoveries which have altered my opinion about Cerdic's ora, for it could so nicely be explained as marking the frontier of the first enclave. I found it quite accidentally about ten years ago when I was bicycling back from Lepe along the old road. It is first visible opposite a house called Forest View (Hants. 83 S.W., edn. of 1909) 300 yards N.W. of the point where the road from Beaulieu to Hythe crosses the Forest boundary. The west end rests on a gully which is the head of a small eastern tributary of the Beaulieu River. Eastwards it surely did not end on the road, but continued the same alignment to the head of a stream running northeastwards

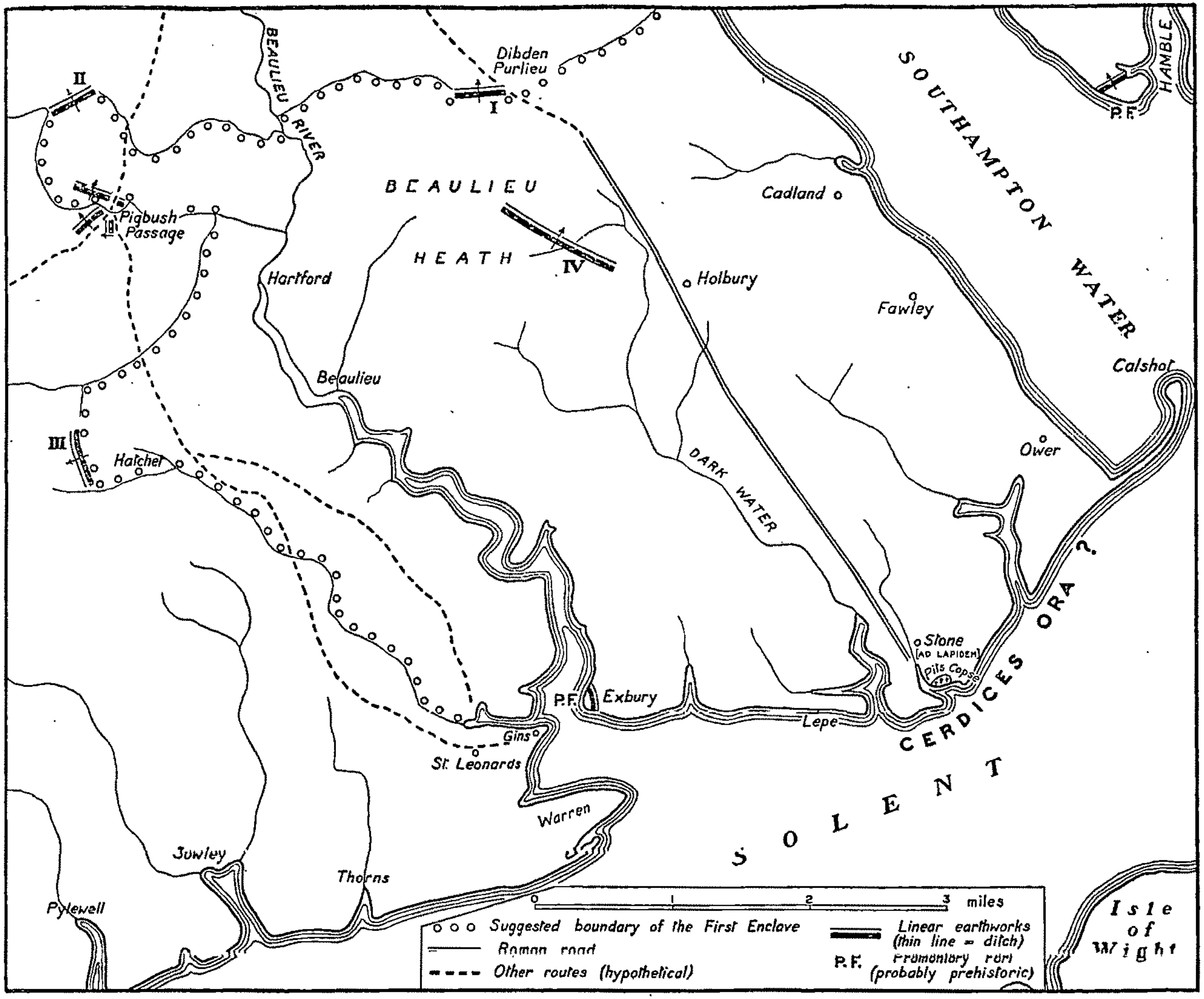

FIG. 2. SUGGESTED ENCLAVE OR BEACH-HEAD DELIMITED BY LINEAR EARTHWORKS

by Langdown to Southampton Water immediately south of Hythe. That stream-head would form the eastern flank of the defence line. The total length of the earthwork would then be a little over half a mile. The ground chosen is on the watershed between Southampton Water and the Beaulieu River, at the point where a narrow col separates them ; southwards the ground at once opens out on to the broad level plain of Beaulieu Heath, to which the col offers the natural way of approach from the north. This earthwork together with its two stream-flanks might well demarcate the territory of primary settlement ; lying across the natural entrance it would be the obvious place to oppose an attempt to enter it by force.

On Beaulieu Heath south of the last mentioned earthwork is another much longer one facing northeastwards (IV, FIG. 2). It is marked on the Ordnance Map (Hants. 73 S.W. edn. of 1909) by a long line, the symbol used to denote modern enclosure-boundaries. The north end rests on some boggy ground which is the source of the Dark Water. In June when I last visited it this ground was distinguished from the rest by the white tufts of bog-cotton seed, a sure sign of wet ground. For 700 yards the earthwork runs quite straight to a bell barrow which it intersects. The overall width of the earthwork in this section is about 20 feet, and the height of the bank above the bottom of the ditch (CD vertical) about 4 feet. At the barrow there is a slight change of direction towards the east, which is maintained without deviation to the end on the west bank of the Dark Water. Soon after leaving the barrow it crosses another branch of the Dark Water below Stonyford Pond, and for 1000 feet it forms the boundary between the parishes of Fawley and Beaulieu. I first saw this earthwork in 1920, and revisited it in June, 1952. It is certainly ancient, and it has the same characteristics as the other three. It is not, I think, a medieval bank, for banks of that age, such as the Bishop's Dyke, usually consist of a very big bank six yards wide made of turf and earth, with quite small ditches on *both* sides. (One of these, also marked by a line in the Ordnance Map, runs northeastwards just N.W. of its N.W. end, to a pond W.S.W. of Flash Pond). There seems no reason for not regarding this earthwork as a subsidiary line of defence of the enclave directed against a possible attack from the direction of Hythe, which may well have been the terminus of a ferry-service from Clausentum.

Another linear earthwork (II) runs across the modern road from Beaulieu to Lyndhurst nearly midway between them and five-eighths of a mile southeast of Beaulieu Road station. The whole of it is preserved, a stretch of 1700 feet, the southwest end resting on boggy ground called Stephill (formerly Steephill) Bottom, and the other on a nameless bog, the head of a western tributary of the Beaulieu River. At a point near the road the overall width is 20 feet and the CD. vertical 5 feet. The earthwork follows the edge of the sloping ground, with a clear view northwards, and the ditch is on the north. The modern road, which is also an old one, leads eventually through Lyndhurst to the Stoney Cross plateau, and its direction is continued by a Roman road whose ultimate destination must have been Sorbiodunum (Old Sarum). The greater part of this earthwork is marked by hachures on the Ordnance Map (72 S.E. edn. of 1910), but it is not named. About the middle of its course it *cuts through* a much older linear earthwork of the bivallate type. Near their intersection the latter consists of a broad deep ditch between two banks, outside each of which is a much smaller narrower ditch. It therefore belongs to the ranche-boundary type usually assigned to the Late Bronze Age, an attribution supported by its more weathered outlines. I traced it southwards to a point inside the Bishop's Dyke, which also intersects it, and northwards to beyond the modern road leading to Ipley. It seems to continue across it northeastwards. The known length is thus about three-quarters of a mile.

PIGBUSH PASSAGE, NEW FOREST

facing p. 197

Nearly half a mile further south is a wood called Pig Bush through which runs an old track to a ford called Pigbush Passage (PLATE) leading across the Shepton Water. Between the wood and the stream are two detached lengths of bank and ditch separated by a furlong's width of boggy ground. Their combined length, with the boggy interval, is 1600 feet; the ditch is on the north and the general direction from W.N.W. to E.S.E. They might be regarded as defences of a bridgehead north of the ford, which may once have been much more used than its present state and position would suggest. South of the ford, between Row Barrow Pond (just inside the Bishop's Dyke) on the west and Halfpenny Green on the east, are two more short lengths of linear earthwork. One follows a sinuous course of about 1000 feet along the S.E. side of the Bishop's Dyke the other, about half as long, runs north and south from near Pigbush Passage towards Halfpenny Green. Their ditches are on the N.W. and W. sides respectively, and they are, together with their banks, of small dimensions, fading out imperceptibly at the ends, except the first one, whose S.W. end is abrupt. These two last seem to have relation to two modern tracks converging on Pigbush Passage from the S.W. and S. The latter may well have been an ancient route of our period, for there seems to have been on an old road of some importance coming from Eling by the modern Beaulieu road to Longdown; here it diverged from the road southwards, leaving the remains of a deep and wide hollow way, in the valley, and below that, the residual causeway.[4] Thence it may have gone over Yew Tree Heath to Pigbush Passage and on to Hatchet Gate, and by Beufre and Ashen Wood to St. Leonard's Grange and Chapel at the mouth of the Beaulieu River. This would be the obvious northern route for anyone landing at or near St. Leonard's on the west side of the Beaulieu estuary. Before the muddy foreshore was reclaimed (probably by the monks of Beaulieu and certainly before 1698) there was a small creek or 'lake' at Gins, half a mile E. of St. Leonard's, at the mouth of the stream from Hatchet Pond, making a haven for small ships. Immediately opposite is the promontory fort of Eberesbury (Exbury) and St. Catherine's Chapel. Gins Farm, of medieval origin, stands on the tip of a gravel spur which would have been an excellent site for a promontory fort. There is in fact a very short length of ditch just east of the farm, between the road and the river-bank on the north, which might possibly be the ditch of a fort of this kind; but the existing remains are too slight to prove the existence of such.

The third of the linear earthworks (III) is one ascribed to the medieval period by Heywood Sumner, but for reasons which I cannot possibly accept. It runs north and south across the road from Beaulieu to Brockenhurst west of Hatchet Pond (FIG. 3) for a length of about 2000 feet resting on boggy ground at each end. It was destroyed during the war to make an aerodrome, but was excavated first in 1941 by Mrs C. M. Piggott, from whose report I quote[5]: 'the excavation, which consisted of a narrow cutting 3 feet wide and 50 feet long, was taken across part of the ditch (now destroyed) about 30 yards south of the road. It showed a roughly scooped-out ditch with only two feet of silt, and a bank to the east of this had been made by first throwing the turfs from the ditch on to the old ground surface and covering these with clay and gravel. An 8-foot length was uncovered along the edge of the bank, parallel to the ditch, while looking for possible post-holes, but none was found'. Mrs Piggott also records that no objects were found. The ditch was on the west and the earthwork therefore faced in that direction.

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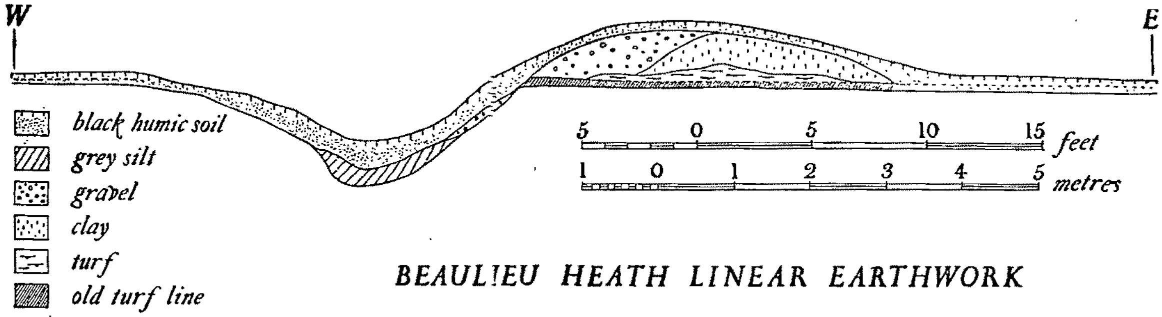
[4] Which closely resembles, but is not, the causeway of a Roman road. I owe my knowledge of this road to Mr L. A. Burgess, Deputy-Librarian, Southampton.

[5] *Proc. Hants Field Club* XVI, 1945, 166. I wish also to thank her and the Editor for permission to reproduce the section, FIG. 3.

These four earthworks (excluding the ones at Pigbush) all seem to be of about the same dimensions and character. Another, near Fritham, which was excavated at the same time as the last, consisted of a bank between two ditches, with the post-holes of a fence or palisade under the bank on the margin of the eastern ditch. Banks with a ditch on each side are usually medieval; there is one such on Setley Plain, and the Bishops Dyke in the Forest and the many boundary-banks at Hursley (one of the same bishop's manors) though far bigger, are of the same character. The Fritham bank is structurally unlike the ones described above and does not fit into the scheme in any way: it may therefore be eliminated from the present enquiry.

If we examine the general disposition of more of these linear earthworks on the map of the district, a very interesting fact emerges; with the streams and boggy gullies on their flanks they form a complete ring round the southeast corner of land between Southampton Water on the east and the Solent on the south. They bridge the cols between those streams exactly at the points where the watersheds were narrowest. The best way of demonstrating this will be to describe the course of a hypothetical boundary which they may be supposed to demarcate.



BEAULIEU HEATH LINEAR EARTHWORK

FIG. 3

We begin at Southampton Water south of Hythe and proceed along the stream by Langdown to its head; then along the dyke (linear earthwork) through Dibden Purlieu to the head of a gully running northwestwards and later turning into a stream running southwestwards across the Beaulieu road to the Beaulieu river east of Buck Hill; thence westwards south of Buck Hill up along a stream between Ferrycroft and Culverley Farm to a dyke, and then southwest along the dyke to Stephill Bottom, and down along it to Shepton Water; down along Shepton Water by Pigbush Passage and Shepton Bridge to the mouth of Wort's Gutter; up along Wort's Gutter to a gully south of Moon Hill; up the gully to the dyke; along the dyke to the south head of Hatchet Pond; along Hatchet Pond eastwards and then down along the stream southwestwards south of Swinesley's, Beufre and Ashen Wood to St. Leonard's and the estuary of the Beaulieu River; then along the shore eastwards to Lepe and Stone, northeastwards to Calshot and northwestwards to Fawley and Cadland to the mouth of the stream south of Hythe where we began. Were this a genuine ancient boundary of the 5th century it might have ended: 'And all the land contained within the aforesaid metes and bounds belongs to Cerdic, king of the West Saxons, by right of conquest'.

In 508 there was a clash with the British king Natan Leod, who was killed; in 514 reinforcements arrived by sea, and in 519 Cerdic and Cynric 'began to reign', and fought a battle at Cerdices ford (Charford). In 527 there was a battle at Cerdices Leah.

Note that many, perhaps most, of the warriors who fought at Cerdices Leah could have been born (many of them probably in Hampshire) after the invasion had taken place; the events so summarily described in the O.E. Chronicle were in fact spread out over a generation. It may help us to realize the slow motion of the Annals if we transpose the dates to our own times. Cerdic and Cynric would have arrived in 1895 when we were going to our prep.-schools; the next recorded event did not happen till we had left college in 1908, and the battle of 1927 found us well launched in our careers. The culminating victory at Old Sarum took place in the present year, 1952. One tends to visualize a quick triumphal march through the New Forest, forgetting that the events were spread out over a lifetime. Is it then legitimate, a critic might ask, to plot the routes of armies at all? Was not the whole thing just a gradual process of expansion? Expansion there was, of course, leading doubtless to friction on the borders and to frontier incidents; but armies as well as people were on the march, battles were fought and new lands acquired. Armies in those days could not leave the beaten tracks which for that very reason were called *herepaths* (war-paths), and it is therefore quite legitimate to try to identify them.

Natan Leod was domiciled somewhere in the vicinity of Netley Marsh which has preserved his name, though the tract of Natan Leah must have comprised much more—probably most of the northern part of the Forest (FIG. 4). It would be natural for him to re-use some existing hill-fort such as Malwood or Tatchbury, just as the Britons of Salisbury Plain occupied the derelict Iron Age hill-fort of Sorbiodunum (Old Sarum). The battle as surely it was, at which he was killed may have been somewhere in the neighbourhood of one of these hill-forts. Natan Leod's defeat would have given the invaders command over a fertile and inhabited plain. The nucleus of this, the second, enclave was probably at Canterton, an ancient manor of pre-conquest age south of Brook. The name (=Cantwara-tun) means the enclosed settlement of the Kentish men and is additional evidence that Cerdic and his followers came from that county. One of the ways out of the first enclave into Natan Leod's kingdom would be along the present northeast boundary of the Forest. An old road ran from Applemore Hill, north of Dibden Purlieu, along that boundary by Ashurst (Lyndhurst Road station) and Bartley to Cadnam, Canterton, Brook and Bramshaw, where it joined—or crossed—the Cloven Way. Applemore Hill is called Dunshamele in the Forest Perambulation of 1279–80[6] which mention a 'via regia' and a 'magna via' between that hill and 'Potteresford', now Poternsford Bridge over the Bartley Water five-eighths of a mile N.W. of Lyndhurst Road station. The first advance northwards may well have followed the line of this road to Canterton. The second, to Cerdic's ford, eleven years later, may have been either along the Cloven Way from somewhere in the neighbourhood of Cadnam Common westwards, or, if it set out from Canterton, along the present Salisbury road (a very old one[7]) from Brook to the point where it met the Cloven Way above Hale. Whichever route the army took it would certainly approach the ford by the Cloven Way which leads down to it.

The battle gave Cerdic command of the ford, and he would be able to cross this wide and important valley and establish a bridgehead on the chalk uplands beyond. The road across the ford continues over Breamore Down to Wick Down where is the defensive linear earthwork which I regard as marking the limit of Cerdic's newly acquired bridgehead. Somewhere on these downs, probably quite near the earthwork was Cerdic's Leah where a battle was fought in 527. Though the name has vanished and identification is therefore uncertain, a faint echo seems to have survived in the 14th century. The Calendar of Patent Rolls for 1338[8] records that Richard de Chardele, servant of Roger de Audele, parson of Fordingbridge, was assaulted at Fordingbridge, and that the parson's close and houses at Wulfedele were broken into. It seems possible that Chardele, the modern form of which would be Chardley, or Charley, may be the Middle English form of O.E. Cerdices Leah; it is certainly the name of a place, and is likely to have been not far from Fordingbridge. A search in manor rolls and other documents (if any survive) relating to the district might be profitable. So far as I know no cartulary of Breamore Priory has survived; if it has, it would be worth searching.

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[6] Cartulary of Beaulieu Abbey belonging to the Duke of Portland (Welbeck 1.A.2, ff. 13, 70).

[7] See ANTIQUITY V, 458.

The erection of defensive earthworks to protect a beach-head is an obvious device dating at least from the Trojan War. It was adopted at Bindon Hill in Dorset by the earliest Iron Age invaders of Wessex about 400 B.C. who landed at Lulworth Cove, as Sir Mortimer Wheeler has just proved.[9] A distinction should be made, however, between the defences of a beach-head (Bindon) and those demarcating the region of primary permanent settlement. Cerdic's round Stone belongs to the latter class; and another example (of unknown age) is found on the south coast of Cornwall where the Giant's Hedge cuts off a piece of coastland 3 miles long between the Looe and Fowey estuaries. The view here put forward is merely a particular application of the accepted one that these linear earthworks were defensive frontiers. One wonders whether its adoption might not help to interpret similar earthworks elsewhere, such as the Danes Dyke at Flamborough Head, some of the Norfolk dykes, and (par excellence) La Hogue Dikke across the Cotentin Peninsula.

It would be nice if the evidence of early place-names could be cited in support of a settlement in this enclave at the date postulated. Unfortunately few if any seem to have survived. Many probably disappeared when the Beaulieu estate was reorganized after the Dissolution; and nearly all the topographical documents of Beaulieu Abbey, which might have recorded the earlier ones, have disappeared. There are three possible *-ing* names; *hyldingbroc* in the bounds of land at *Celces ora* (Calshot) in A.D. 980,[10] probably the Dark Water or Stanswood stream, or one of their tributaries; Keeping Copse near Buckler's Hard (1698); and perhaps a copse now called Walton Copse near Holbury, called Wanting Coppice on the Tithe-map.

The purpose of this article is to record some new discoveries and air some new theories. It supersedes my previous article in respect of the site of Cerdic's ora and the first part of the march to Cerdic's ford. But my account of the course of the Cloven Way still holds good, being a record of observed facts. I have not the slightest doubt that it was in use throughout its length as a thoroughfare between Totton and Charford and on beyond it into Cranbourne Chase. But if my new view of the site of the landing is correct, the Cloven Way is unlikely to have been used by the invaders except at its western end above Charford. One other point needs mentioning; when I wrote the first article I did not make enough allowance for the slowness of the advance, treating it too much as if it were a single campaign instead of a gradual process of conquest and settlement spread out over a generation.

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[8] *Cal. Pat.*, Ed. III, vol. 4, p. 150 (1st October, 1338).

[9] The account of his excavations there is to be published by the Royal Commission on Ancient Monuments (England) in their Dorset volume. We give the above facts with Sir Mortimer Wheeler's permission.

[10] Kemble, *Cod. Dipl.*, No. 626.

Some Aspects of American Culture-history: a review

by GORDON R. WILLEY
(*Harvard University*)

THIS, the third volume of RUNA*, is a journal of general anthropology for the professional reader. The emphasis is Americanist, but a range of interest covering all fields of Old and New World archaeology, ethnology, folklore, physical anthropology, and theory is indicated in the selection of brief contributions and book reviews. Notably absent is anything on the social or applied anthropological side. The volume, which is well set up typographically and pictorially, contains 4 long articles, 6 short contributions, 22 book reviews, lists of publications, and miscellaneous notes. Most of the reviews are by the editor, José Imbelloni, or by O. F. A. Menghín, staff member of the Instituto de Antropología.

This is a review of the major articles which deal with archaeological subjects.

Investigaciones Prehistóricas en Cuevas de Tandilia (pp. 5–36) (by O. F. A. Menghín and Marcelo Bórmida). Since Ameghino's day the problem of early lithic finds has been of unusual interest in Argentina. Although the putatively ancient remains of the Buenos Aires littoral failed to stand up under careful scrutiny, recent discoveries in the Argentine and other parts of southern South America have offered substantial proof of pre-ceramic periods. The cave remains which Drs Menghín and Bórmida describe from the hills of Tandilia in southeastern Buenos Aires Province are among such evidences. The Tandil caves, Gruta del Oro and Gruta Margarita, are rock ledge formations originating from water action in the late Tertiary or early Quaternary stages. Test excavations in the Gruta del Oro revealed a fourfold physical stratification with cultural remains in the two lower layers. The topmost layer contains diatoms which resulted from a period of moist climate believed to have been the sub-Atlantic phase of the area. The second layer (from the top down) was partially composed of volcanic materials and is thought to correspond to a relatively dry period. The third layer is a humus zone filled with chipped stone artifacts. It is assigned to the Atlantic phase, another humid interval. The fourth and basal stratum shows dry conditions accompanied by vulcanism. Artifacts are also found in the upper portions of this soil zone. Total depth of the excavations averaged 1.50 metres.

Dating of the sub-Atlantic phase is 1000–0 B.C. The previous dry interval is estimated as 2500–1000 B.C. while the Atlantic humid period is given the dates 5000–2500 B.C. The earliest stratum antedates the 5000 B.C. mark. This geological dating places the artifacts, which are found in the third and fourth layers, somewhere in the neighbourhood of 4000 to 6000 years before the beginning of the Christian era.

The artifacts are knives or scrapers chipped from chalcedony or quartzite. Most of the implements show work along only one edge. One tool type appears to be a flake form with a beak or point, and there is a core with some retouch which may have been used as a scraper. Virtually all identifiable artifacts came from the Gruta del Oro, the Gruta Margarita showing the same physical stratigraphy but yielding no definite stone forms.

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* *Runa, Archivo para las Ciencias del Hombre, Vol. III, pts.* 1–2. Instituto de Antropología, Facultad de Filosofía y Letras, Universidad de Buenos Aires. 1950. (303 pp., various plates, figures, maps. Price not stated).

Menghín's comparisons begin with the specimens from a nearby cave, Ojo de Agua. This site, together with the Gruta del Oro, are the basis of a *Tandilense Complex.* The Tandilense complex is then compared with the remains from another cave, Cueva Eberhardt, where similar stone tools were found associated with some rather unspecialized bone artifacts. In matching Tandilense-Eberhardt with Bird's sequence in the Straits of Magellan, Menghín holds out for a Period 2 cross-tie, although he admits that the Magellanic complex is much richer, especially in bonework. In this connection he makes the very interesting suggestion that Bird's Period 2 may have been derived from the north from a Tandilense base and that in the Straits region it had been modified by other elements coming from the Period 1 level of Bird's stratification. Menghín goes on to state that Tandilense is a part of a world-wide "*Proto-lithic Bone Culture*" with technological survivals from the Lower Palaeolithic.

It seems to me that a pre-ceramic complex of about 6000–4000 B.C. is a reasonable expectation for the Argentine Pampas. Here, as elsewhere, the position of Holmes and Hrdlicka on early man in the Americas is no longer defensible. It also seems likely that other lithic complexes will be revealed in Argentina and that these will demonstrate a continuity from the Tandilense (or possibly something earlier) up into the pottery period. Rigorous prosecution of such sequences is still the great need. Menghín and Bórmida have made a valuable, geologically well-documented contribution toward the solution of these sequence problems. It is to be hoped that the Tandilense complex will be expanded by further investigations of this type. As to the 'Protho-lithic Bone Culture' and world-wide diffusions, such concepts will be accredited when the comparisons of a series of stratigraphic sequences throughout the areas or continents in question disclose similar technical complexes in equivalent chronological contexts.

Teotihuacan, Tula y Los Toltecas. Las Culturas Post-Arcaicas y Pre-Aztecas del Centro Mexico. Excavaciones y Estudios. 1922–1950. (pp. 37–70) (by Pedro Armillas). This account of Teotihuacan, its chronology, and its relationships with another important upland Mexican site, Tula, is a sharply defined synthesis of an extremely complex situation. I believe that it will rank as one of the great summary expositions of Middle American studies. Certainly it will be a necessary point of departure for all future writings on the Teotihuacan-Tula problem.

For the past 20 years Teotihuacan has been a keystone in the chronological structure of Mexican and Central American archaeology. Its carved tripod vases and polychrome fresco jars are the horizon markers of the Classic Period from Peten Maya to the Guatemalan highlands and Oaxaca to the Veracruz coast. Hence, the culture sequence at Teotihuacan proper is of utmost importance. Recent excavations at the site have brought in new information to be correlated with, and to modify, Vaillant's Teotihuacan sub-periods. Further, the excavations at Tula, Hidalgo, have thrown light on the history of Teotihuacan and on the Toltecs who were once, erroneously, associated with the Teotihuacan metropolis. The Armillas review incorporates these new data in a convincing manner.

Armillas begins by summarizing native mythology and history surrounding Teotihuacan making it clear that the builders of the site cannot be identified satisfactorily with any ethnic group. He follows this with a summary of archaeological explorations from Batres to the recent work by the Mexican Instituto de Antropología. Then, he branches out to show the distribution of Teotihuacan culture and of Teotihuacan influence throughout Middle America. Switching to Tula he discusses the Coyotlatelco-Mazapan-Tenayuca pottery stratigraphy and its relationships to the final period at Teotihuacan. His last section, and the substance of his contribution, is the chronological summarization of Teotihuacan. In this he dispels much of the cloudiness with which the Teotihuacan sub-periods had been enshrouded. In the chronology which Armillas offers he has, wisely, forsaken numerical designations for sub-period or phase names: thus, any further splitting of the recognized sub-periods, or additions of new ones, if such is found necessary, will not be hampered by a closed numerical series.

The earliest of Armillas' Teotihuacan sub-periods is *Chimalhuacán*. This is a late Archaic manifestation equatable with Noguera's El Tepalcate finds and serving as an intermediate link between Ticoman and the pottery from the fill of the Pyramid of the Sun. As such, *Chimalhuacán* antedates Vaillant's sub-period I. *Tzacualli*, which follows *Chimalhuacán*, is the equivalent of Vaillant's I and is the late Archaic occupation on the Teotihuacan zone. *Tzacualli* is terminated by the great pyramid building marking the inauguration of the *Miccaotli* phase of the Classic Period. During *Miccaotli* the Pyramids of the Sun and Moon, the ' Street of the Dead ', and, possibly, the Pyramid of the Plumed Serpent within the Ciudadela were constructed. The phase corresponds to Vaillant's sub-period II figurines but not to the carved tripods or fresco pottery which he also placed in II. *Xolalpan* succeeds *Miccaotli*. The *Xolalpan* phase saw the expansion of the urban zone to a maximum of 750 hectares. Most of this increase was in residential units. *Xolalpan* relates to Vaillant's sub-period III but also contains certain elements which he attributed to II and IV. Significantly, this is the period of the tripod and fresco ware and of the famous naturalistic murals. It is also the period of the greatest expansion of Teotihuacan influence throughout Mesoamerica. *Tlamimilolpa* is placed as subsequent to *Xolalpan*. It is reasonably close to what Vaillant had in mind with his sub-period IV. Champlevé ware, carved before firing, is characteristic. Some residential groups were built at this time on the periphery of the city. The sub-period is brought to a close by the burning of at least part of the urban area. Coyotlatelco ware is found over the burned ruins. *Ahuitzotla-Amantla*, Armillas' final sub-period, is named after the western barrios of Azcapotzalco. This is Vaillant's sub-period V, and it is scarcely represented in the Teotihuacan zone, proper, but seems to be a refugee survival after the destruction of the city.

One of the most striking inferences to be drawn from the Armillas summary is this. If the *Xolalpan* phase is correlated with the first half of the Maya Initial Series chronology (and such a correlation is well-supported by *Xolalpan-Tzakol* ceramic cross-finds), we must conclude that the apogee of Teotihuacan civilization was achieved somewhere between A.D. 400–600 (Thompson correlation) or well in advance of the Maya Great Period. If this is true, the pyramid building of the *Miccaotli* phase, which includes the construction of the Pyramids of the Sun and Moon, antedates A.D. 400 and, probably, the beginnings of the Maya Initial Series. In other words, at a time when the lowland Maya were building no imposing structures, or at best something resembling the small Uaxactun pyramid of E-VII-sub, the inhabitants of the Valley of Mexico were constructing a mammoth ceremonial centre and may have been well on their way toward large urban populations.

La Extraña Terracota de Rurrenabaque (Noreste de Bolivia) en la Arquelogía de Sudamerica. (pp. 71–169) (by J. Imbelloni). Dr Imbelloni's long essay is a *tour de force* in the realm of ceramic diffusion in the Americas. The subject of the title is a pottery urn in human form which was found on the Beni River, near Rurrenabaque, in lowland Bolivia. The Rurrenabaque specimen serves simply as the starting point for a survey of human pottery representations in South American and, to some extent, Middle American archaeology. The author set for himself the objectives of locating the geographical foci of primary and secondary cultural diffusions and of tracing the paths of these diffusions throughout Andean and lowland South America.

Near the close of his essay Imbelloni makes an interesting observation. This is to the effect that archaeologists have been preoccupied largely with a study of *styles* but have ignored the study of *types*. This necessitates some explanation, particularly for North Americans, who associate somewhat different meanings with these terms from those implied by Imbelloni. It has been customary for many North American archaeologists, as well as for some Europeans working in the American field, to use the terms pottery *style* and *type* synonymously or to consider types as subdivisions of major styles. Imbelloni's usage differs (and he has a precedent in Montelius) in that he conceives of a type as a broad descriptive category which may be cross-cut by various styles. As an example, the Andean vessel form, the *kero*, is a type ; but this type may be found in many styles such as the Coast Tiahuanaco, the Chancay, or the Inca. Style, in his usage, is the particularism of materials, technique, and pattern of design—a definition comparable to that given it by most other South American archaeologists. His type, on the other hand, is an abstraction more on the order of what I have called a *tradition*, a continuity through history of certain modalities which may be expressed in different styles. Leaving terminology aside, Imbelloni's point, that the type (or tradition) is an important working concept in the unravelling of culture history, still stands. I am in agreement with this outlook for it has long seemed that the American archaeologist is up against serious difficulty in attempting wide-scale reconstructions of cultural interrelationships solely upon the basis of stylistic similarity. The cross-referencing of one area with another is often blocked by stylistic regionalism. In order to discern cultural relationships across great geographic distances it will be necessary to have recourse to broader typological categories than styles. The effectiveness of these broader categories (Imbelloni's type) is, of course, limited by an important factor. This is that the form of the type must retain sufficient distinctiveness so that it is always a recognizable unity in spite of the various stylistic modifications which it may undergo. Without this recognition and distinctiveness we are left in doubt as to whether diffusion has been operative or whether the similarities which we see are the result of convergent development.

Imbelloni's case that the pottery human effigy form has a common history in the Americas leads inevitably to Spinden's concept of an ' Archaic horizon ' and the New World beginnings of human figurine modelling in Central Mexico. Imbelloni fully appreciates the difference between a specific time horizon and the traditional continuance of an idea, and makes clear that he is dealing not with a remote, widespread, and uniform Archaic epoch but with the diffusion and persistence of a what he believes to be a single type. In his opinion this type, the plastic representation of the human figure, had its primary South American centre of dispersal in Colombia and Venezuela. From this focus the type split in its diffusion. A western arm, characterized by a predominance of figurine forms, extended down the Andes and Pacific Coast. The eastern arm depending from the Colombian-Venezuelan focus spread to the South American lowlands and established secondary centres along the Amazonian system. Here, the human plastic representations tended to take the form of urns rather than figurines.

In his comparisons Imbelloni proceeds from the smaller to the large geographical units. On his first level of operations he considers four groups of pottery: (1) coastal and highland Ecuador (human figures with or without benches); (2) the Atlantic coastal-Maracá region (seated human figure urns); (3) the Ecuadorean-Peruvian montana (effigy urns of the Rio Napo); and (4) the Amazonian course (effigy urns of Miracanguera, Marajó, and Yapurá). All of these groups he considers as derivative of his primary Colombian-Venezuelan centre, and there is certainly suggestive evidence of a possible common parentage in such a north Andean focus. The relating of the Colombian-Venezuelan primary centre, with its main Andean and lowland bifurcations, to Archaic origins in Middle America is the final step of the hypothesis. I follow in agreement with this interesting reconstruction only part of the way. There is substantial accord that the tropical lowland urn figures from Bolivia to the Maracá coast are linked in a strong traditional sense. This must be admitted in spite of the fact that we know almost nothing of their relative time positions. From Colombia-Venezuela there are also urn figure connections with the Amazonian groups, and, although it has not yet been proven, I would now incline toward Imbelloni's view that the primary diffusions of the human effigy idea were from the Andes to the lowlands. In highland Colombia and Ecuador there are suggestive similarities between such pieces as the Popayán (Colombia) human figures seated on benches and the Amazonian plastic representations. Further relationships in this direction appear less secure. The coastal figurines of Manabí, Esmeraldas, and Tumaco are highly specialized, and it is at this juncture that one begins to doubt Imbelloni's use of his type concept in plotting lines of a grand unitary diffusion. For as recognizable unity of the type begins to blur, alternative explanations for the presence of human effigies of pottery on the Ecuadorean coast deserve to be taken into consideration.

In other words, I do not believe that the broad *type* or *tradition* conception can be carried rigidly to its logical extremes. Although the limited geographical distribution of stylistic entities will certainly force the archaeologist to modify his methodology in attempting to demonstrate long-range cultural relationships, he cannot eschew chronology altogether. The *type*, as defined by Imbelloni, is susceptible to temporal treatment even though this may not be as finely calibrated as the sequence studies of styles. The lack of such treatment or the consideration of its possibilities is, I feel, the weakness of Imbelloni's method. He is quite definite on this point when he states:

> '. . . *the investigation of sites where there exist great richness and stratigraphic variety, cannot in a single way be identified—by whatever method—with the investigation of large regions whose characteristic is the dispersion of some few forms*'. (italics Imbelloni's) (Author's Translation) (p. 100).

It is evident that the geographical tracing out of a form (type) over an extensive area is separate as an immediate operation from the chronological plotting of a given form in the stratigraphy of a specific site; nevertheless, the history of a form within a large region can be most effectively comprehended if we know the relative time positions of its various occurrences in that region. In no other way can we be certain of its direction of geographical movement or its sequent modifications. Archaeology has as its dimensions not only *form* and *space* but a third dimension of *time*.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

THE CELTIC FIELD-SYSTEMS ON THE BERKSHIRE DOWNS, by P. P. RHODES. *Oxoniensia* XV, 1950 [1952], 1–28. [A thorough and detailed survey, fully illustrated by large-scale maps, for which we must thank the Council for British Archaeology. A model of its kind].

TERRA-SIGILLATA-GEFÄSSE DES ERSTEN JAHRHUNDERTS MIT TÖPFERNAMEN, by ROBERT KNORR. W. Kohlammer, Stuttgart. *D.M.* 12.

FOUR HUNDRED CENTURIES OF CAVE ART, by L'ABBÉ H. BREUIL. Centre d'Études et de Documentation préhistoriques, Montignac, Dordogne, France. 5200 *fr.* to subscribers, 6200 *fr.* to the rest.

THE EXCAVATION OF TWO ADDITIONAL HOLES AT STONEHENGE, 1950, and new evidence for the date of the monument, by R. J. C. ATKINSON, STUART PIGGOTT and J. F. S. STONE. *Antiq. Journ.* XXXII, 1952, 14–20. [Radiocarbon date for charcoal gives 1848±275=2123–1573 B.C.].

THE EXCAVATION OF KNACKYBOY CAIRN, St. Martin's, Isles of Scilly, 1948,by B. H. ST. J. O'NEIL. *Ibid*, 21–34. [Important finds of beads].

THE STANWICK EXCAVATIONS, 1951: Interim Report, by R. E. M. WHEELER. *Ibid.* 1–13.

STRATIGRAPHIE DU LOESS et évolution des industries paléolithiques dans l'ouest du bassin de Paris, by F. BORDES. *L'Anthropologie*, LVI, May 1952, 1–39.

RADIOCARBON DATING, assembled by FREDERICK JOHNSON. Supplement to *American Antiquity*, Vol. XVII, no. 1, part 1 (July 1951). Society for American Archaeology, Salt Lake City, Utah, U.S.A. $1.50.

RADIOCARBON DATING, by WILLARD F. LIBBY. University of Chicago Press (Cambridge University Press, England). £1 6*s*.

THE HITTITES: a summary of the art, achievements and social organization of a great people of Asia Minor during the 2nd millennium B.C., as discovered by modern excavations: by O. R. GURNEY. Pelican Books, 3*s* 6*d*. [A long-needed book about a neglected region].

THE KARASUK CULTURE AND ITS SOUTH-EASTERN AFFINITIES, by K. JETTMAR. *Bull. Mus. of Far Eastern Antiquities*, Vol. XXII, 83–126, 16 plates.

THE ALTAI BEFORE THE TURKS, by K. JETTMAR. *Bull Mus. of Far Eastern Antiquities*, Vol. XXIII, 135–223, 36 plates.

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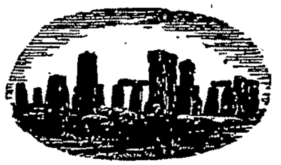

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Notes and News

A CHINESE PUZZLE

In July, 1940, the Editor received, through a correspondent, information about a megalithic structure in China which outwardly seemed to resemble those European monuments which used to be called 'dolmens'. The times were not favourable for further investigation, but we have now followed the matter up, with the following result.

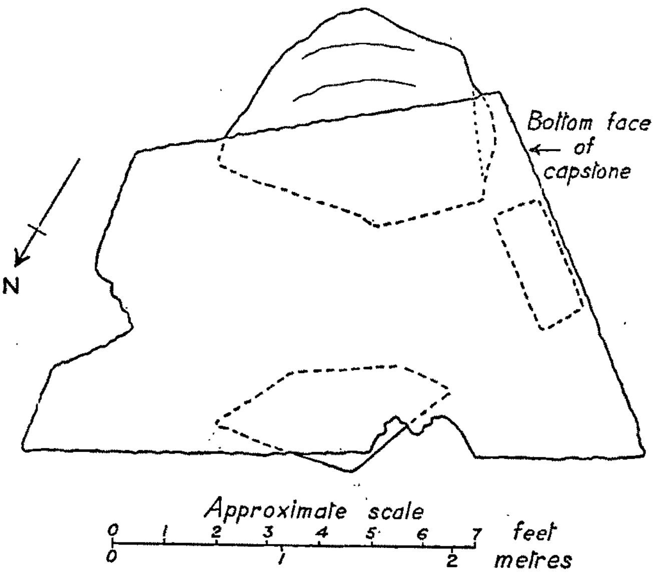
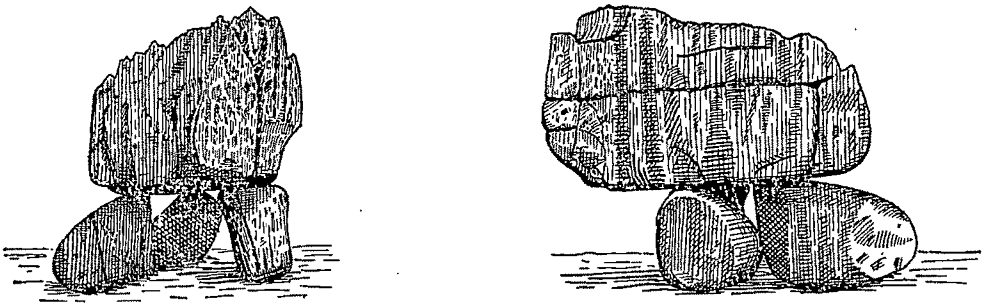


FIG. I. PLAN OF BLOCKS AT HWANG T'IEN P'U IN S. HUNAN

The structure was discovered by the Rev. Robert S. Harrison, who was then residing at Lingling (Yungchow) in S. Hunan. It is situated at Hwang T'ien P'u in the Lingling district, and the approximate coordinates are Lat. 26° 15′ N., Long. 111° 40′ E. In his original letter of 1940 Mr Harrison quite properly pointed out that the structure might be of natural origin. It consists of a huge block (or capstone), resting on three smaller uprights between 3 feet 3 inches and 3 feet 6 inches high. The capstone is a little over 5 feet thick and about 7 feet square. The N.E. and N.W. sides are more weathered than the others, and the top and sides have been eroded into grooves by the action of rain-water. A piece of one of the uprights had been chipped off by workmen in need of metal for road-repairing. Locally it is regarded as the work of fairies and of immemorial antiquity. It is not associated with any religious rites or regarded with any reverence. At Mr Harrison's request the district magistrate, though regarding it without interest as being the work of prehistoric barbarians, promised to protect it.

The structure stands on a natural limestone platform 16 feet from E. to W. and about 50 feet from N. to S. This base is in two steps, the lower being 3 feet 2 inches (W.)

by 2 feet 1 inch (E.), and the upper 1 foot 9 inches (W.) by 1 foot 7 inches (E.). One of the photographs accompanying the note (not reproduced because not good enough) shows in the background a natural hillock of limestone whose two lowest strata seem to consist of slabs similar both in size and character to the uprights and capstone of our 'dolmen'; it seems therefore not unlikely that the latter is in fact merely a residual fragment of a similar hillock, the rest of which has been removed, partly by natural and partly perhaps by human agency. The problem could only be solved by inspection on the spot, preferably by someone with geological field-experience. The uprights should be examined to see whether their stratification is horizontal or vertical; if the latter, they must obviously have been set up by human agency. A comparison with the lowest stratum in the adjacent hillock would probably settle this point. We are inclined to favour a natural origin, and we hope that this Note will not be quoted as evidence of 'dolmens' or megaliths in China, where so far none have been recorded. We publish this Note because, as the plan and drawings made from the photographs show, the structure does closely resemble a 'dolmen'.



VIEW FROM N. VIEW FROM S.W.

FIG. 2. VIEWS OF BLOCKS

We wish to thank Mr Harrison for supplying the materials for this Note, and the Rev. H. Garner who drew our attention to the structure in 1940; also Mr Frank Addison, F.S.A., for making the drawings. O.G.S.C.

EUESPERIDES—A DEVASTATED CITY SITE

The accompanying plate (facing p. 210) is an enlargement of a vertical air-photograph of the outskirts of Benghazi (Cyrenaica), taken a few years ago by the R.A.F. for routine purposes. It covers the area immediately adjoining a large Moslem cemetery situated at the junction of the Benghazi by-pass and the Benghazi-Tocra road, two miles north-east of the modern city centre. This cemetery was constructed early in the Italian occupation of Cyrenaica, was enlarged shortly before the British occupation, and is likely to expand still further in future years. It occupies rising ground which divides the two salt-marshes, Sebka Ain es-Selmani and Sebka ez-Zreriia (FIG. 1). In ancient times these marshes were lakes connected by a narrow channel immediately west of the cemetery, and communicating also with the present inner harbour of Benghazi, and with the sea itself. They thus provided a sheltered haven for shipping.

The photograph reveals the town-plan of an ancient city situated on the edge of the marsh between the cemetery hill and the channel previously referred to. It reveals it, however, in ' negative ' form, as a site thoroughly devasted by stone-robbers during late Turkish and early Italian times. The ancient walls appear dark because they are today open robber-trenches, with vegetation growing in their bottoms. The ancient streets appear as light strips, because they survive as raised causeways, into which the stone-robbers have had no reason to dig. The general plan of the city is none the less clear, especially at its western edge where the robbing is most recent : and, on this side, one can still make out the outline of the city-wall, with projecting towers.

These features are hardly recognisable on the ground, without the photograph in hand as a guide. The casual visitor can discern only a meaningless maze of overgrown trenches and fragmentary mortar floors, hedged in on one side by the cemetery, on another by a dump of scrap-iron salvaged from the desert battlefields, and on the third by the marsh itself, which dries out in summer exposing a surface of sickly pink salt-encrustation. No site could appear more squalid and hopeless at first glimpse.

Yet the whole area of the ancient city is littered with Greek pottery, much of it of remarkably fine quality[1], and members of the Map of Roman Libya expeditions[2] visiting the site in 1950 and 1951 collected a more or less representative selection, which has recently been studied by Mr Peter Corbett of the British Museum. Mr Corbett reports that the bulk of the pottery is Attic, and belongs to the 5th and 4th centuries B.C. There was a single sherd of the 3rd century B.C., but no Roman wares at all. The general later limit for the group seems to be the third quarter of the 4th century, and the latest sherd may be a stray from the cemetery hill, where there is reason to believe that occupation continued later than on the main site[3].

This ancient city-site, the existence of which appears to have been unknown (or, at least, unpublished) before the recent war, cannot be *Berenice*[4] which took its name from the wife of Ptolemy III Euergetes in about 247 B.C., and was continuously occupied in Roman and Byzantine times, the emperor Justinian rebuilding the city walls. Indeed, it is quite certain that *Berenice* lay beneath the centre of modern Benghazi. Mosaics and walls were found during the construction of the modern buildings by the Italians, and tombs of the Hellenistic and Roman periods have come to light at Sidi Hussein and Mafluga, on the edge of the inner harbour, between Benghazi and Berka[5]. Significantly, nothing of pre-Ptolemaic date seems to have been found in Benghazi itself[6].

Yet *Berenice* had a predecessor, *Euesperides*, which was in existence as early as 510 B.C., and had to be relieved by a Greek naval force in 413 B.C., owing to the attacks

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[1] To the best of my knowledge the presence of Greek pottery on this site was first reported by Mr Frank Jowett in 1947 ; and Mr T. Burton-Brown, then Antiquities Officer in Cyrenaica, immediately recognised the importance of the site and took steps to prevent further encroachments. The air photograph came to my notice only in 1950.

[2] Mr John Eames and Mr David Smith were both active in examining the site, at my suggestion, during these expeditions.

[3] Mr C. N. Johns has informed me that pottery of rather later date has come from the Moslem cemetery area during recent levelling.

[4] For the earlier history of *Euesperides* and *Berenice* see E. S. G. Robinson, *B. M. Catalogue of Greek Coins of Cyrenaica* (London, 1927), CLXXXVIII.

[5] E. Ghislanzoni, *Notizie Archeologiche della Cirenaica* (Rome, 1915), contains a good account of discoveries made in the first years of the Italian occupation. Later discoveries are largely unpublished.

[6] The important 4th century B.C. inscription, referring to *Euesperides* by name, recently published by Mr P. Fraser (*Bulletin de la Société Royale d'Archéologie d'Alexandrie* 1951, pp. 132–43), is of uncertain provenance, but may have come from the site under discussion

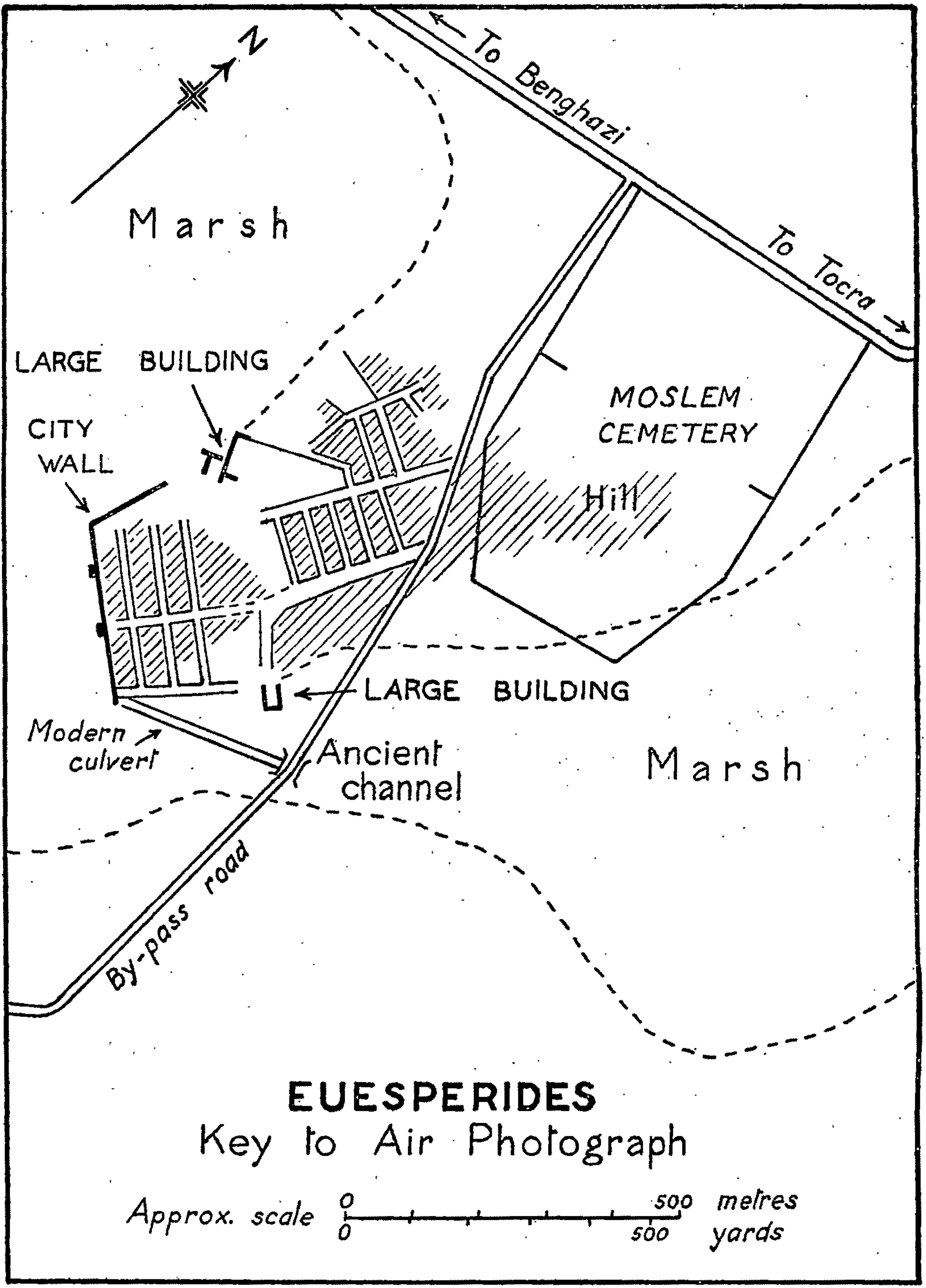
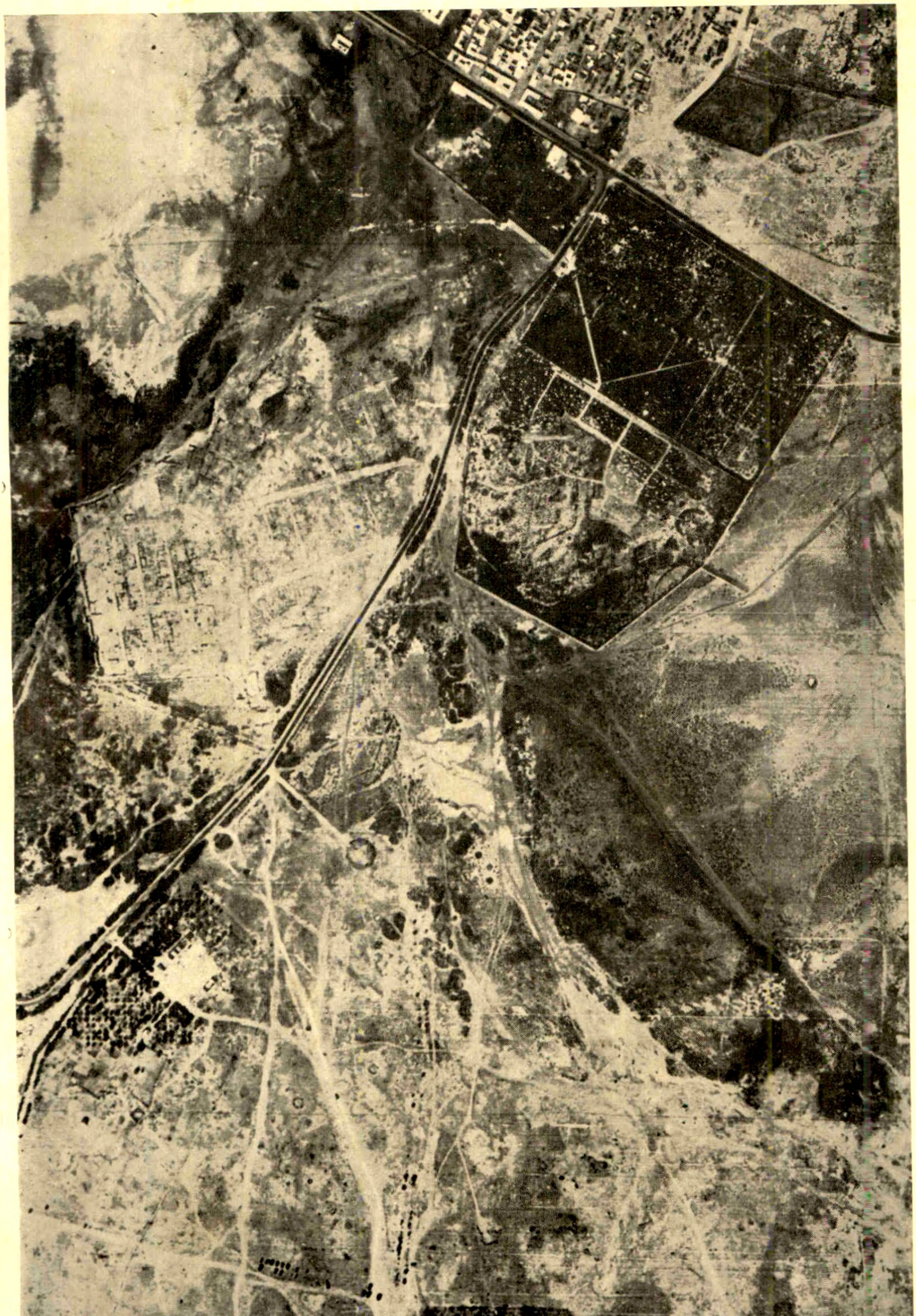

N
To Benghazi
To Tocra
Marsh
LARGE BUILDING
CITY WALL
MOSLEM CEMETERY
Hill
LARGE BUILDING
Modern culvert
Ancient channel
Marsh
By-pass road
EUESPERIDES
Key to Air Photograph
Approx. scale
0
500 metres
0
500 yards

FIG. 1



AIR PHOTOGRAPH, TAKEN IN 1949, OF THE SITE OF *EUESPERIDES* (see Key, opposite)

Photo by courtesy of Air Ministry

of Libyan tribesmen. It must be this earlier city that appears, in ‘ ghost ’ form, on our air photograph ; and from the pottery evidence already cited it would seem that the main city site was shifted nearer the sea even before *Berenice* received its dynastic name. The reasons for the change of site can only be guessed, but the drying-up of the inner Sebkas, and the consequent loss of the original harbour, is the most likely guess. In Strabo’s time[7] the new city had already been long in existence, and the geographer makes no mention of a predecessor ; but Strabo does refer to ‘ a certain lake Tritonias, in which the principle things are an isle and a temple of Aphrodite ’. It has always been agreed that the Sebka Ain es-Selmani must represent Strabo’s ‘ lake ’, and the only feature approaching an ‘ island ’ is, in fact, the promontory of rising ground on which the Moslem cemetery now stands. Thus the temple of Aphrodite was probably a sanctuary of the old city of *Euesperides* which remained in use long after *Berenice* was founded. Statues of Aphrodite have, in fact, been found in the vicinity of the *Euesperides* site[8].

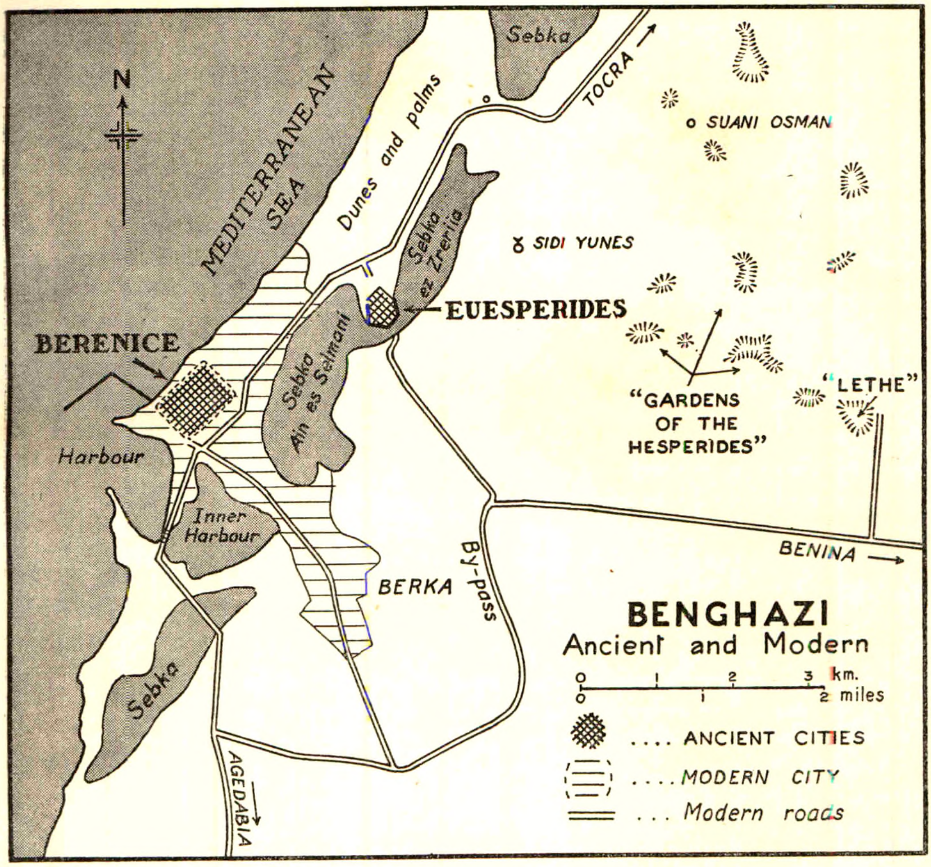


Fig. 2

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[7] Strabo, XVII, 3, 20.

[8] Ghislanzoni, op. cit., p. 7.

Strabo also refers to the ' Harbour of the Hesperides ' (probably the lagoon of the Sebka ez-Zreriia) and to the ' River Lathon '. The famous ' Gardens of the Hesperides ' in this same area have been convincingly identified with the series of small natural depressions which lie north of the Benghazi-Benina road. In the largest of these depressions, near which King Idris of Libya now has a palace, there is an underground pool which probably gave rise to the fable of the ' River Lethe ', but there is, in fact, no real underground watercourse anywhere in the area, although a high-level water-table permeates the whole region. Now that the true site of *Euesperides* is known, one can see how close to the city were the famed ' Gardens '.

Mutilated as it is, *Euesperides* is hardly likely to attract tourists and scholars away from the better-known ruins of Cyrene ; but it offers the archaeologist a magnificent opportunity of finding early Greek levels close to the surface and without Roman intrusion. The walls of the town buildings have gone, but the floors which they surrounded remain, in some areas, intact. The proximity of the cemetery—and, indeed, of modern Benghazi—counsels urgency in the investigation of this important site. It is therefore particularly reassuring to learn that Mr C. N. Johns, the present Controller of Antiquities in Cyrenaica, began preliminary excavations at *Euesperides* in the autumn of 1952, under the auspices of the Ashmolean Museum, Oxford. R. G. GOODCHILD.

JEBEL ANAZEH IN JORDAN, IRAQ AND SAUDI ARABIA

A low hill amid flint and gravel-strewn terrain marks the focal point of three countries. A cairn of limestone blocks built on the highest point is the southeastern corner of the Hashemite Kingdom of the Jordan, the southwestern angle of Iraq and the most northerly point of Saudi Arabia.

Jebel Anazeh (Enaze, Anaiza) is also famous in this great wilderness, for from its southern flank a narrow gully leads down to the undulating plain ; this is the origin of the great Wadi Hauran which meanders northeast past spotlevel 2301 to El Mat just north of Rutba Wells to join the Euphrates south of Haditha.

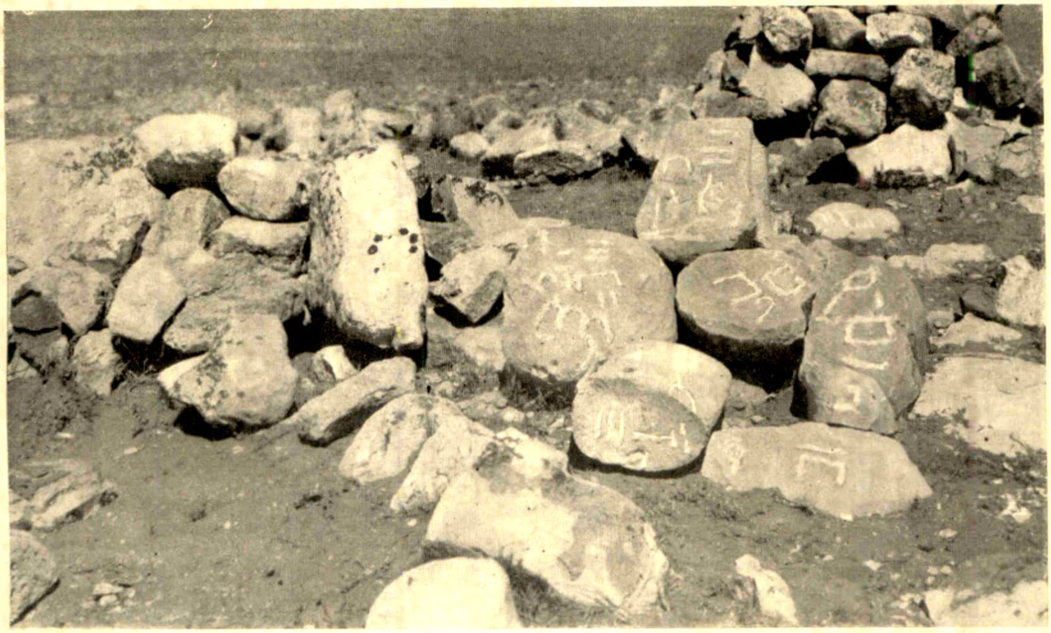
In March, 1928, as leader of the Second Field Museum North Arabian Desert Expedition I was privileged to accompany the Iraq Petroleum Company's Survey Party under W. E. Browne, who was searching for the best alignment for the proposed pipeline from Kirkuk to Haifa. Our purpose was to record archaeological data, particularly surface Stone Age sites.

On Jebel Anazeh flint stone implements of Middle and Upper Palaeolithic types were found, indicating that this modern international landmark was visited by the Prehistoric hunters. At that time the Wadi Hauran flowed with water, for we traced camp-sites along its edges for a hundred miles.

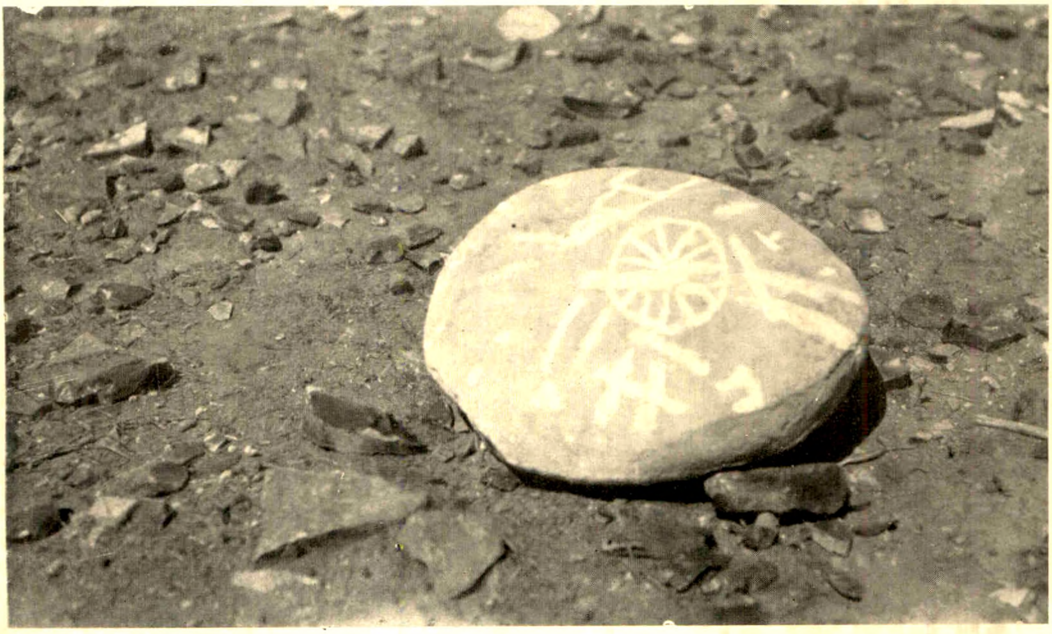
To the southeast on top of Umm Wual a fine series of deeply-patinated Mousterian implements were collected.

In June, 1951, during the Peabody Museum-Harvard Expedition I found Palaeolithic implements on the summit and eastern slopes of Tell el-Hibr, some thirty airline miles to the southwest in Saudi Arabia. On Jebel Anazeh in 1928 I collected implements of honey-coloured flint which must have come from Tell el-Hibr. This flint fractures easily and is reminiscent of the ' livres de beurre ' of Grande-Pressigny, one of the first articles of commerce in Western Europe.

Beduins visit Jebel Anazeh on their way to grass or to the wells at El Jidd, El Mat and Rutba. They bury their dead near its summit. They hammer their tribal marks (*wasm*, pl. *wusum* or *wasmat*) on the limestone boulders to record their passing.



a. CAMEL-BRANDS HAMMERED ON LIMESTONE BLOCKS JEBEL ANAZEH



b. CAMEL-BRANDS AND GRAFFITI ON LIMESTONE BOULDER, JEBEL ANAZEH

Photos by Field Museum North Arabian Desert Expedition, 1928

NOTES AND NEWS

Before the Nairn Overland Desert cars roared through the ‘ blue ’ on the Jerusalem-Amman-Rutba-Baghdad run or the Cairo-Baghdad Air Mail hummed overhead, the Beduins, usually guided by a Sulubbi, hunted the ostrich from Tellul Basatin midway between Jebel Anazeh and Jebel et-Tinf (the central south Syrian-Iraq boundary point) as far south as the Laha Depression, the last sanctuary of the Arabian ostrich in this area.

Near Jebel Anazeh we drove over the top of a low escarpment to see a herd of 300 gazelle kicking up puffs of dust as they dashed madly away from the roaring monsters which disturbed their privacy.

In these days Jebel Anazeh is quiet for it lies off the direct path of the ever-flowing oil. Its fame is assured for it appears on every map of Southwestern Asia.

HENRY FIELD.

Reviews

EXCAVATIONS IN MESOPOTAMIA AND WESTERN IRAN : Sites of 4000–500 B.C. Graphic Analyses *by* HENRY WARE ELIOT, JR., Art Work *by* THERESA GARRETT ELIOT. *Special Publication of the Peabody Museum, Harvard University, Cambridge, Mass.*, 1950.

Most of the results of fifty years' digging in the Tigris-Euphrates valley are still scattered among various periodicals and preliminary reports ; to find out what has been dug up at Uruk-Warka one has to turn up eleven 'vorlaufige Berichten' and three special monographs ; 'Ur Excavations' provided articles for *Antiquaries Journal* for ten years, only a small fraction of which can now be replaced by the two volumes of the British Museum official publication. Reports on Susa are still more widely dispersed. As a student and in preparing the Nuzi pottery for publication, the late H. W. Eliot, like the rest of us, had to summarize for himself the often contradictory reports and make shorthand copies of the published figures of types he needed to remember. But he saw that this immense preparatory labour, imposed on all scholars, is an intolerable obstacle to the progress of knowledge, and that to relieve his colleagues and successors of this burden would in itself be a contribution to knowledge. He found in his wife a competent draughtswoman who would collaborate and who could translate the rough mnemonic sketches all of us make in reading such reports into neat line-drawings that for practically every purpose are serviceable substitutes for the original published plans, figures and even half-tone plates. In 1947 untimely death cut short the execution of his well-designed project. The major part of the section devoted to Babylonia had, however, been completed, and this the Peabody Museum has now made available to his colleagues in a handsome atlas.

In publishing Eliot's work so sumptuously the Museum and University have earned the gratitude of all students of Near Eastern archaeology and history, and have ensured to the author a reputation no less honourable than any he might have earned had he 'devoted those years to writing books and articles' instead of providing 'the information that would enable others to write' as Lauriston Ward puts it in his Foreword. The atlas consists of seventeen double-sided sheets of stiff card, 2 ft. by 1½ ft. Each page, called an 'Exhibit', usually combines plans and line-drawings with a typescript text, both reproduced photographically with complete success. Seven exhibits are devoted to Uruk-Warka (the Erech of the English Bible), six to Ur, four to Kish, two to al'Ubaid and one to Jemdet Nasr, but no less than ten to Susa. A comprehensive map of sites throughout the Near East (which means in American today what it did in English in the days of Hall and Hogarth) precedes the exhibits, while a comparative table of selected ceramic and metal forms from type sites follows the rest. We are told that the author had planned to include Babylon and Lagash (Tello) and may assume that, had he lived, he would have treated Eridu, Shuruppak and Uqair too.

An exhaustive bibliography, going back to the days of Loftus and Dieulafoy, is included in the exhibits dealing with each site, and will be an immense convenience to specialists who must of course bear in mind the date of completion ; neither the XIth vorlaufige Bericht on Uruk-Warka nor Mémoires XXIX and XXX of the Délegation en Perse seem to have reached Harvard in time for inclusion. No less convenient are the comprehensive plans of all sites ; the general plan is all too often missing from the particular heavy tome one happens to be reading ! A very wide and judiciously chosen selection of objects—vases of clay, stone and metal, metal tools, weapons, ornaments, tyres and harness-fittings, seals, pictographic tablets and even such sculptures as the Lion Hunt stele from Erech and the Code of Hammurabi from Susa—is admirably reproduced though generally without indication of scale. Brick sizes are recorded in all cases. Objects are described in connexion with the layers to which the excavators most often attribute them—they sometimes contradict themselves—but the text gives a critical appraisal of the excavation reports which shows how unsatisfactory the stratigraphical evidence often is. The author does his best to summarize the published evidence objectively and yet extract some consistent picture from it; in the case of Susa the conflicting reports on the several *sondages* are set out side by side in parallel columns! Sometimes the scepticism thus engendered may have carried the author a little too far; there is really no need to regard as intrusive in the 'Jemdet Nasr' layer of the 'Acropole–Sondage I' at Susa the copper shaft-hole adze which might very appositely have been figured on the last exhibit to emphasize the continuity between Uruk-Jemdet Nasr and Early Dynastic. Still these sceptical warnings are necessary in a summary too concise to reveal fully the shortcomings of oriental excavation and of its publication, and the student is free to accept or reject the excavator's statement in their light.

In fact Eliot offers his readers much more than a mere illustrated summary of excavators' observations and deductions. His critical analysis of the material summarized has inspired constructive suggestions. Many of the original contributions are modestly hidden, but stimulating heresies on chronological and terminological questions are boldly set forth and argued. It will be remembered that as late as 1925 the 'Pre-Sargonic' or 'Archaic Sumerian' era still faced directly the prehistoric age of painted pottery, best known then from al'Ubaid. Then in 1926 Langdon's excavation of Jemdet Nasr disclosed a culture with painted pottery that was epigraphically and stylistically older than 'Pre-Sargonic' but later than al'Ubaid. Next in 1930, Jordan, exploring the E-anna at Erech, exposed in layers -IV to -VI a series of temples with pottery, seals, and tablets different from, and epigraphically earlier than, those from Jemdet Nasr which he assigned to a Uruk period; in 1931 Heinrich and von Haller reached in a pit below the temple floors strata -VII to -XII containing unpainted pottery superimposed upon those containing al'Ubaid wares and assigned them too to the Uruk period. Finally in 1934 Frankfort proposed a tripartite division of the 'Pre-Sargonic' into Early Dynastic I, II and III based on the stratigraphy of Tell Asmer and Khafaje.

Now Eliot quite rightly insists that the material from the several 'Uruk' levels in the E-anna is far from homogeneous; the pottery from the deep levels in the pit represents a different assemblage from that recovered from the temples of -V and -IV, and the latter comprises many types that recur at Jemdet Nasr. The disparity within the old Uruk period is indeed a fact which other authorities have independently recognized as needing terminological expression. Seton Lloyd gives it recognition by a distinction between Early and Late Uruk. Delougaz confines the term Uruk to the sort of material collected from the pit in the E-anna and assigns the temples of -V and -IV and cognate pottery and seals to a Protoliterate Period *a* which is not yet Jemdet Nasr. On the last page Eliot notes with approval Delougaz' proposal, but has already on Exhibit VI frankly assigned the temples, seals and tablets to the Jemdet Nasr period, confining the term Uruk to material from lower levels in the pit. He goes on there to argue that in terms of the historical traditions preserved in the Sumerian King List his Jemdet Nasr ought to be called Early Dynastic I. He does not apply the latter terminology in the remaining exhibits, but in dealing with Susa he does use Jemdet Nasr in the extended sense he defended on Exhibit VI.

Admittedly the polychrome pottery that was first used to define the Jemdet Nasr period is poorly represented in the published material from Warka and indeed from any site in Sumer ; much of the material from the Flood Pit at Ur, described by Woolley as Jemdet Nasr in 1930, would, if published, have been labelled Uruk in 1940. Uqair has since provided plenty of typical polychrome pottery, stratified above unpainted Uruk, but Uqair is in Akkad. On the other hand quite a number of unpainted pot-forms from Jemdet Nasr can in fact be matched in the old ' Uruk ' levels at Warka. Of course Uruk is distinguished from Jemdet Nasr not only by ceramic types but also by artistic styles. But these seem to offer a rather subjective criterion. At Warka itself several objects, including the Lion Hunt stele and some seals from the *Sammelfund*, actually found in the ' Jemdet Nasr ' level –III, have been assigned to the Uruk period and explained as votives preserved from earlier temples. In dealing with the seals Eliot concludes that Frankfort's ' rule appears to be to assign those of superior quality to the Uruk period and inferior ones to Jemdet Nasr on the theory that glyptic deteriorated '. He suggests himself that the real differences in style may be due as much to divergences between artistic schools as to disparities in age.

The divisions of Early Dynastic are subjected to the same sort of criticism. Following Harden, Eliot can show the close continuity of types both within Early Dynastic and between it and Jemdet Nasr and Uruk. The point is brought out graphically in the table of pottery types on p. 33 which, however, likewise shows that the differences, if small, can easily be grasped even from figures. Only at his peril can an archaeologist accept a couple of ceramic or metal shapes as type fossils, sufficient in themselves to date an assemblage ; periods will have to be defined statistically by the relative frequencies of types, but material for such treatment is seldom available in the publications. In any case the chronologist may be discouraged by the continuity revealed from exaggerating the length of the several periods. Perhaps Delougaz' ' reasoned guess ' of 1000 odd years for Early Dynastic and Protoliterate is not so ' reasonable ' after all. On the other hand it may only mean that four—or five-thousand years ago Sumerians changed their fashions a great deal more slowly than Americans of today. The hint, first given in Exhibit VI but repeated on p. 33, that the transition from Late Uruk to Jemdet Nasr may really be ' dynastic ', deserves serious consideration. If Erech were then in some degree an ' imperial city ', the magnificence of the temples in –V and –VI would cease to be surprising. If the epigraphic conventions then adopted by the temple clerks became the medium of communication of an empire, their general adoption throughout Babylonia, but not in Elam, could be explained. But Jacobsen's ' primitive democracy ' would have to be pushed back into the remote Ubaid period while current accounts of the Urban Revolution in Mesopotamia would need drastic revision.

V. GORDON CHILDE.

EARLY MAN IN THE NEW WORLD. *By* KENNETH MACGOWAN. *Macmillan, New York* 1950. 251 *pp., text* 218 *illustrations, Index and Bibliography. Price* 37*s* 6*d.*

Mr MacGowan is to be congratulated on what is undoubtedly a very useful introductory text book to American prehistory, as are the publishers on a very well produced and well illustrated book. Mr Campbell Grant's black and white illustrations are particularly good especially those illustrating flaking techniques and objects and uses of ethnographical specimens. The obvious care that the author has taken in the choice and presentation of his tables and illustrations has resulted in a very happy corroboration of the text. For example the pages embodying the four main stages of the Wisconsin glaciation, the repetition of time scales on different pages in the chapter on Old World prehistory and chronology and the tables of the important sites of Early Man in the New World, the diagram to illustrate the solar radiation theory to explain the Pleistocene glaciations, facilitate reference and make the text even clearer—if this is possible. The publishers may perhaps be criticised for putting the selling price of the book quite as high as they have, as this may act as a deterrent to some who would otherwise have purchased the book.

Mr MacGowan's text is easily readable and for a book which contains so many facts and theories it is remarkable how he has contrived to make it so enjoyable to read. One does not feel that the stone tools, extinct fauna, and human remains which he describes are dead and lifeless things to be treated as geological fossils and nothing more; rather one obtains a very clear picture of the life of the Folsom bison hunters, or the Cochise food gatherers, and the environment in which they lived.

The underlying question right through the book is of course the age of Man in the New World and from where he penetrated to the American continent. The author gives due consideration to the work of many others in this subject, and one must admire the way in which he gives to each theory, however fantastical, the consideration it deserves, so that we can follow him the whole of the way to the conclusions which he draws. Not a little of the value of the book comes from the fact, however, that the author does not draw didactic conclusions where no such conclusions are yet possible. The facts are reviewed with all care and their interpretations by the different workers are set out, but we are very rightly often left to draw our own conclusions. While at the time this book was written many still have doubted that the antiquity of Man in the Americas reached back to the end of the last glaciation, the development of the C14 radiocarbon method of dating has since remarkably confirmed the view of the majority of American prehistorians that he must go back at least to this time, perhaps earlier.

The author discusses all the possible migration routes and shows that the Arctic Circle route from Siberia across the Bering Strait to Alaska was the only really practical one. His chapter on the Great Ice Age is a useful summary and prepares the stage for the description of all the more important sites where fossil man and his tools have been discovered. His chapters on Old World prehistory may perhaps be criticised on several points, the main one being that he appears to be under the impression that Mortillet's classification still holds its own in the minds of many prehistorians in different parts of the world. Mr MacGowan has as one may say, gone out of his way almost to over emphasize that this classification cannot now be applied with exactitude anywhere outside a small part of Western Europe. He appears to be unaware that, for example, workers in African prehistory, have not attempted to apply this classification in their continent since the early years of the century. Quite rightly Mr MacGowan points to the dangers of universal time scales, but one would have preferred to have seen less emphasis on this point.

The rest of this chapter is a useful summary of the established Western European cultural succession, with a fuller consideration of the Solutrean, induced quite naturally because of the early American pressure flaked points.

The author may be criticised for his retention of the cultural term ' Yuma '. Radiocarbon dating has now confirmed that the continued use of this ' omnibus ' will have to be discontinued as unjustified, containing as it does several distinctive types of point. When describing the various early American cultures the author confines himself almost entirely to consideration of the ' type fossils '—the now well known ' points ' of the Folsom hunting-groups and the grindstones of the food gatherers. One would have liked to have known more of the rest of the material culture indicated by scrapers and cutting tools and the rest of the stone industries.

An important chapter discusses the great extinction of the Pleistocene fauna and the possible causes of this. The different physical types of Man as indicated by fossil man are considered. The book ends with a discussion on the origin of the cultures of Early Man in the Americas—whether they are due to diffusion or independent invention. The book entirely fulfils its purpose which is to provide an introduction to the study of Early Man in the New World; and it makes very enjoyable reading.

J. DESMOND CLARK.

ANATOLIAN STUDIES. Vol. 1. *Journal of the British Institute of Archaeology at Ankara. Published by the Institute at* 56 *Queen Anne Street, London,* W. 1. 1951.

Archaeologists will give an even warmer welcome than is usual to a new periodical to Volume 1 of *Anatolian Studies*. It is the first periodical in English devoted to the study of the archaeology of the area covered by the modern Turkey. It is a record of the activities of the newest of the British Schools of Archaeology overseas, and in welcoming it we congratulate Professor Garstang, the prime begetter of the Institute. It is a pleasantly produced volume of good format and print, and for this we congratulate the Editor, Professor Childe. It includes reports of excellent work already accomplished by the Institute, and for this we congratulate the Director, Mr Seton Lloyd.

The volume contains a summary of archaeological research in Turkey, 1949–50, which is invaluable for reference, and which shows the wealth of work in progress there, all too little known hitherto in this country. There are reports of the Institute's work at Polatli, valuable for its excellent stratigraphical succession from the Chalcolithic to the Hittite periods and the detailed publication of the pottery therefrom, and at Harran, a site of importance as early as the time of Abraham, but where the present work was concerned mainly with the remarkable medieval remains, and on Mr Sinclair Hood's excavations at Tabara el Akrad, which are especially useful for a study of Khirbet Kerak ware. The matter of this first volume should bring many new subscribers to the Institute.

A few blemishes are noted, in the hope that their future omission may improve the already first class publication. Why, with the present cost of paper are there two blank pages? The diagram on p. 33 reaches the extreme limit of legibility, and the names on the plan of Harran pass it (quite unnecessarily, as there is plenty of space). And for the drawings of pottery, which are in themselves excellent, might one plead for the blacked-in sections usual in British archaeology, and the omission of the bounding lines? By forming the edge of the figure with the edges of the drawings one saves a lot of space, and the unpleasing overlapping of drawings might be avoided. The sections of stratification in the Tabara el Akrad report leave much to be desired.

These are small points, and one would wish to end on a whole-hearted welcome and praise for an excellent publication. K.M.K.

POTTERY FROM THE DIYALA REGION. *By* PINHAS DELOUGAZ (The University of Chicago, *Oriental Institute Publications, vol.* LXIII). XXII+182 *pp.*+204 *plates, Chicago,* 1952.

The Oriental Institute's excavations at Khafaje, Tell Asmar and Tell Agrab yielded the first complete series of stratified relics from Jemdet Nasr (here termed 'Protoliterate c-d') to Sargonid times, and on them Prof. Frankfort based his three-fold division of the Early Dynastic period that had hitherto been treated as an indivisible 'Pre-Sargonic era'. Now the ceramic documents are fully published and illustrated with 16 coloured plates, 116 half-tone plates, and 83 plates of section drawings. The immense mass of pottery, so thoroughly studied by Delougaz, confirms the conclusions reached by Frankfort in 1936 and introduces only minor modifications. In the 'Pre-Sargonic' it has been possible to distinguish a fourth phase, termed the Protoimperial and equivalent to the century beginning with the accession of Entemena of Lagash. Scarlet Ware, recognized by Frankfort as distinctive of Early Dynastic I, may in fact outlast that period; in particular Delougaz argues, but on typological rather than stratigraphically certified analogies, that the famous chariot vase in the British Museum should be classed as E.D II. The well-documented series of Early Dynastic types should provide a standard of comparison with the aid of which unstratified sepulchral finds from other Mesopotamian sites may be more precisely dated; Delougaz himself makes some suggestive and startling comparisons with the Royal Cemetery at Ur. But in practice his very original system of classification is a serious obstacle to such use of the material.

The author was faced with an unprecedented volume of material to arrange, and to deal with it he has evolved, as Prof. Frankfort says in the Foreword, 'a completely new type of classification'. It is indeed strictly logical and yet simple, precise (being based largely on metrical qualities) and capable of use by a novice. But, in contrast to Petrie's 'empirical system', it fails to bring together types that are related chronologically or genetically. The new type of classification is in fact modelled on the Dewey decimal system of library classification, enlarged by prefixing A, B, C, D or E to the six numerals to indicate absolute size. The first digit indicates basic geometrical forms or simple combinations of such, the next modifications or further combinations, the third proportions, and the last three features like base, neck, spouts and so on. (It must be remembered that the bulk of the material is plain drab wheel-made ware so that peculiarities of fabric and decoration can be and are discussed outside this frame). In his descriptive text Delougaz does not use his own system but treats together types that have empirically been found to be significantly related under descriptive terms like 'four-lugged jars', 'spouted jars', 'upright-handled jars'. But in the catalogue section which contains details of provenance, number of examples, section drawings and comparisons with other sites, these groups are dismembered, the objects being arranged first in sizes (all A sizes together), and then according to the generalized forms indicated by the first digits. As a result 'four-lugged jars' appear under A.604,223; A.633,263; B.455,253; B.663,2-3; C.493,253; C.634,253; D.504,253 and at intermediate points. On the other hand plate 187 containing 'forms C.653–C.655' presents twelve vases that have no visible relationship and chronologically are dated 'Protoimp.'; 'ED.III'; 'Protolit.d'; 'Protolit.c'; 'ED.III'; 'ED.II'; and even 'E. Larsa'. To trace the variation with time of, say, the spouted jar or to ascertain the relative frequency of a variant in a given period the reader must therefore go through in turn all the pages of Bs, Cs and Ds fixing his eye on the last digit in the columns. At the same time this system precludes the author from presenting a full statistical analysis of the distribution of types by layers; for only complete or restorable vessels can be classified under the first two or three decimals of his system though spouts, which are exceptionally durable, and even their variants could have been almost fully enumerated. V. GORDON CHILDE

NOTES ON ARCHAEOLOGICAL TECHNIQUE. Third Edition. *Ashmolean Museum Publication, Oxford* 1950. 25 *pp. Price* 1*s* 6*d.*

The publication of the latest edition of this small handbook for amateur archaeologists, first issued in 1940, is now entirely the responsibility of the Ashmolean Museum, incorporating revised sections by members of the Oxford University Archaeological Society and others.

The book is intended primarily for the excavator of rescue digs—sites uncovered by commercial or other undertakings which will necessarily be destroyed. The writers quite rightly stress that skill and proficiency as an excavator can only be attained by experience ; but it might perhaps have been stressed in particular that the best way of obtaining this is to work for one or more seasons under an experienced and professional excavator. It might even be said that only by, as it were, serving his apprenticeship in this manner will an excavator obtain the true experience that is the very essence of good excavation.

The notes contained in this handbook are written primarily for the excavation of British sites and can be applied to most small open sites dating from the Mesolithic period upwards.

The introduction would bear careful reading more than once by the excavator approaching the subject for the first time as it rightly stresses that excavation is essentially hard and exacting work calling for much care and patience and often not a little endurance, and that once a site has been excavated it is lost for ever so that it is for the excavator to extract from the site the fullest possible details.

Sections are included on initial arrangements, tools and equipment, the excavation proper, recording of finds, plans, maps, sections, drawings and photography, with a special section on the treatment of pottery, presumably because this is considered to be the most common form of find that will be made.

Some of the advice, such as ' do not walk through or trample down standing crops ' would seem to be hardly necessary ; but for the most part these notes are just such as should prove useful to the amateur. The section on excavation is clear and full of important advice which should always be followed. Due stress is laid on the stratification of a site, though the digging of layers, even deep ones, in spits of 6–9 inches is perhaps rather unwise, in the initial stages of the dig certainly. The importance of an accurate and complete record is given the emphasis it deserves and particulars of the kinds of records that should be kept are given.

The section on photography is valuable, though in the reviewer's experience, when dealing with a site which is to be completely destroyed it is a little unwise to place reliance on as few as two photographs of any subject especially if there is any delay between exposure and developing.

The handbook closes with a short section on the final report on the excavation rightly pointing out that it might as well never have been undertaken if the results are not published—a point, incidentally, not only for the amateur archaeologist. A short bibliography giving information on further books for the general reader is also available. These Notes will obviously prove of value to would-be archaeologists in the same way as the earlier editions have proved to have been.

J. DESMOND CLARK.

ARCHAEOLOGICAL RESEARCH IN INDO-CHINA, VOL. II: The district of Chiu-Chên during the Han dynasty. *By* PROFESSOR OLOV JANSE. *Harvard University Press*, 300 *pages*, 43 *plates*, 144 *figures in the text, and one general map.*

The first volume of the author's work has already been reviewed by the writer in the September 1951 issue of ANTIQUITY, pp. 118 to 122, and, in view of the detailed treatment of the contents of vol. I, we shall here be content to mention merely a few of the finds which may be of a special interest to students of the culture of the Han period. Most of the finds in the many tombs excavated and studied by Prof. Janse have been briefly treated in vol. I but in vol. II they are subjected to a closer examination besides being compared with similar objects in the neighbouring countries, such as China, Champā, Cambodia, Siam and even in far away Japan and India. Indeed it is interesting to observe how the two main currents of ancient Chinese and Indian civilization and art meet and mingle in the vast peninsula of Indo-China which, just because of this blend of cultures, might well, and with greater reason, be called Indo-China than Further India.

Prof. Janse, whose reading seems to be encyclopaedic as regards the Far East and other parts of Asia, is a careful worker, and his reports on the finds in the numerous tombs excavated by him are detailed and the description precise, setting an example of how such research work should be carried out. As already said, the Vietnamese are *au fond* Indonesians who, resting on a basis of a now vanished Melanesian population, have suffered inroads by Môn-Khmers, Thai and, especially, Chinese. All of them leaving a strong linguistic and cultural imprint on the Vietnamese. An intensified study of their material and spiritual culture would perhaps reveal traits inherited by present-day Vietnamese from all of their predecessors, the primitive Melanesians included.

In his preface to vol. II the learned author points out that the finds in the Han tombs not only do throw light on the hitherto little known Southern sphere of the classical Chinese civilization but, in addition, the study of certain unique pieces (f.i. of bronze lamps and lampadairs, pottery types, beads, etc.) seem to reveal commercial and cultural relations with both the distant Indo-Iranian world, and perhaps even with the shores of the Eastern Mediterranean, which is of particular interest for the study of the diffusion of human culture. In this connection the author very aptly remarks that there is ample evidence that, when it comes to the study of the past, we have often to think in terms of oceans and continents. A third volume, treating the excavations made at the ancient Indonesian dwelling place at Dông-Sön, will be published by the Institut Belge des Hautes Études Chinoises (Brussels). This third volume will complete the publication of Prof. Janse's excavations of the Han monuments in the Than-hoa province in North Annam. It will be remembered that it was in the district of Dông-sön that a rich bronze culture, dating back to the middle of the 1st century A.D., was discovered by French archaeologists already in 1924. The finds included both Chinese and, what now must be termed, Indonesian ones. Among the latter were the famous bronze drums decorated with scenes depicting the voyage of the dead across the ocean to the Happy isles).[1] Similar scenes are represented in the pictures of the primitive Dayaks in British North Borneo which shows the ethnic and cultural relationship of the ancient Dông-sönians with the populations of Insulinde. The text of this second volume of Professor Janse's is divided into 16 chapters ; each chapter treating a particular group of mounds with the subterranean tombs and their funerary deposits. At one place in Northern Than-hoa were found no less than 30 mounds of which 27 were excavated. It seems that a flourishing Chinese colony existed here formerly. As already said in the review of vol. I the majority of the tombs were built of bricks, funnel or tunnel-shaped, with vaulted roofs, only in some few cases were the roofs or the entire tomb built of wooden slabs, since rotted away.

The orientation of the tombs was evidently not regarded as important, for some are oriented N.–S., others N.E.–S.W. or E.–W. The bricks used are generally well made, though of varying size, and often decorated with various incised patterns. Such decorations of bricks were common in ancient Champā (the hinduized Indonesian kingdom in Annam), as well as in Cambodia. The author's comparisons of the Han tombs in

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[1] *Vide* Victor Goloubew, ' L'Age du bronze au Tonkin et au Nord Annam '. *B.E.F.E.O.*, vol. XXIX, 1929, and his explanation of the frieze on the so-called Curtis vase in the Musée de Louvre, Cahiers de l'École Française d'Extrême Orient. No. 30 (1942), page 23.

Annam with ancient tombs at Lô-yang in Honan and in Shantung, and even in distant Korea, show how widespread and uniform was the old Chinese culture. Besides the bronzes and ceramics already described in vol. I there were also found, in a few places some silver rings and gold drops. Precious metals were, as a rule, absent but they may have been stolen in the many tombs rifled by sacrilegious treasure hunters. The ceramics are often decorated with the quatrefoil pattern. Many of the bronze objects found are of a very large size such as cauldrons, bowls and jars. The numerous mirrors, lamps and lampadairs, also made of this alloy, easily surpass the rest of the finds in interest by reason of their artistic shapes and decoration. The lamps or lampadairs often take the shape of kneeling human figures carrying the lamp-stands. As these human figures have quite un-Chinese features (with wavy hair, or even curled), besides by reason of their lack of clothing, they no doubt represent Indonesian individuals, to wit Chām people. Such figures have been called lamp-carrying Chām prisoners. They may, however, represent certain of their princes taken prisoner by the invading Chinese. In Note 105, p. 46, the author talks of the possibility of Etruscan lamps finding their way via the Near East (Parthia) out to the Far East in the 1st century A.D. In this connection Prof. George Coedès' find of a Pompeiian bronze lamp at the ruined Indian settlement of Phŏng Tük in West Siam may be recalled.[2]

Some of the bronze lamps are in the shape of three-legged birds with a dragon as handle. The most curious object of bronze found was that of a statuette of a kneeling man (un-Chinese of features and in dress) who is holding a tray in both his hands. Eleven gnomes are placed at various places on his body ; some of them beat cymbals or play on flute whilst others are grasping three twigs, which are attached to the arms and the back of the main figure, the twigs being used as lamp carriers. The meaning of this statuette has been the object of much speculation, the late M. Przeluski opining that the figure represents the Giant Purusa and the figurines common men. However, the kneeling figure may also represent a Lokapala, a king of the Yaksas, and the late M. Goloubew suggested that its prototype came from Mathurā in India to Champā from where it was adopted by the Chinese colonists in North Annam. Another bronze figure, found at Dông-sön, holding a spear in his hand, is called a ' barbarian ' because of his curly hair and scant clothing. Lamp-carrying figures are also found in Cambodia and Siam taking the shape of Apsaras (heavenly dancers) and elephants respectively ; but these are of a much later origin than our above-mentioned figures. The author also discusses the meaning of the tree or twigs that are attached to statuettes. He thinks that they may represent the cosmic tree, the tree of knowledge and of life, or the solar tree with its sun birds, which is a conception hailing from the West. He also relates the Chinese fable about the ten suns that threaten to burn up all life on the earth, and how, by order of the mythical Emperor Yao, a divine archer shot down nine of the suns thereby saving mankind. This myth is well known among the savage Moi or Khā of the Annamite cordillera. There is a continuation of the Chinese fable (not known to the Khā people), according to which the divine archer was rewarded for his superb archery with an elixir of immortality which was tasted by his disobedient wife. As a punishment she was exiled to the moon. Here she is visited every fifteen days (the lunar phases) by her husband who lives in a palace in the sun. The latter part of this fable seems somewhat construed in order to explain a natural phenomenon. However, the ideas connected with a tree of life and knowledge and the disobedient woman seem to be but a faint echo of the tale related in the Book of Genesis. The above mentioned bronze figures are also compared with certain stone sculptures of the Chām art such as the cross-legged sitting bearded ascete wearing a pointed head gear which bears an undeniable likeness to the bronze statuettes thereby confirming the Indonesian inspiration of these. Among the finds was also a ' magic ' cauldron, self-boiling and self-producing food which leads the thought onto the old Nordic fairy tale of ' Boil little pot '. The author remarks that the huge bronze cauldrons in the Imperial palace in Hué are symbolic of the Imperial dynasty's power (see also the magic kettles of the ancient Chou dynasty).

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[2] *Vide* George Coedès, ' The excavations at Phŏng Tük and their importance for the ancient history of Siam ', *Journal of the Siam Society*, vol. XXI, part 3.

Among the funerary goods were earthen steam-pots and bowls for boiling soup of just the same shape as their modern counterparts in China and Indo-China. Weapons and implements of iron were but rarely found in the tombs though the iron age was well on its way during the Han period, and only once was a complete skeleton found. The many bronze mirrors were often found lying on the very spot where the hands of the dead person probably had been. Were such finds of mirrors not an indication that the deceased was a woman ? It is curious that among the many ceramics used as lamps some are of the same shape as those seen in the reliefs in Singhasari (Java) and of those found in the 25 B.C. necropolis at Susa ! It seems that the prototype of the ceramic lamps of the Far East may be sought for in the Near East. A unique find was a glass egg which may symbolize the resurrection. Such eggs are also found in Viking tombs in Sweden (A.D. 800–1050). Our Easter eggs also symbolize resurrection. The earthen mounds covering the tombs are generally very low, a few reaching a height of 4 metres, and are thus not to be compared with our Nordic ' Giants Hills '. Only in one tomb were the trappings of a horse found. This might mean a horse sacrifice as at the burial of great Chao Fà princes in Shanland. Among the iron swords found one was ornamented with an appliqué startlingly similar to that seen on a Merovingian sword ! Though not many of the graves excavated by Prof. Janse were untouched the sum total of the finds made was nevertheless very rich and instructive. Prof. Janse also examined some kilns from the Han period containing thousands of pot-sherds and glazed tiles besides jars, bowls, vases and even funeral jars. ERIK SEIDENFADEN.

SCRIPTA MINOA. The Written Documents of Minoan Crete with Special Reference to the Archives of Knossos, *by* A. J. EVANS. Volume II, *The Archives of Knossos.* Clay tablets inscribed in Minoan Script B *edited from Notes and supplemented by* J. L. MYRES. *Published for the Oxford University Press by Geoffrey Cumberlege* ; VI + 102 *pp.*, 5 *tables*, 82 *collotype plates.* U.K. *Price* £10 10*s* 0*d. net.*

It is a difficult and somewhat thankless task to edit another man's work many years after his death, but rarely can such a work have been carried out with more loving care than this volume. The ' editing and supplementing ' by Sir John Myres represent no light work since the scope of it varies from that originally planned by the author. Pugliese Carratelli's publication of the Hagia Triada tablets rendered further work on them unnecessary for the present. The cost of publication had risen greatly. The editor therefore decided not to publish in detail the Linear Script A texts (not very numerous if one excludes those already published), but to concentrate on a full and complete publication of the tablets of Linear Script B, which in Crete is peculiar to Knossos though more widely distributed on the mainland. Volume II however does include the photographic plates of the B tablets, which under Evans' original scheme would have been left for Volume III. The first 36 pages explain the characteristics of the two linear signaries with lists of the signs common to both and those peculiar to each.

To the 'new material' mentioned on p. 36 may be added the Apodhoulou stone vase with a linear A inscription, and the axe from Arkalokhori and the unwrought stone from Mallia (both with hieroglyphic inscriptions). The next 30 pages describe Script B with suggested uses and significances for individual signs. Evans deserves the greatest credit for his restraint in refraining from speculation on linguistic parallels and phonetic equivalents, and for that and other reasons his work will continue to be valuable when the ingenious speculations of Dr Hrozny have joined the still more imaginative interpretations of Mr Gordon in the limbo of 'forgotten fairy tales'. Some of the most valuable features of this book are the concordance which enable us to correlate plates and line drawings with the numbers in Evans' list and with those of the Herakleion Museum inventory. A smart piece of detection by Myres reveals the disturbing fact that the tablets published from the Giamalakis' collection, were apparently at an earlier date in the Herakleion Museum !

The printing, line drawings and plates are all admirable and the work will be indispensable to all archaeological libraries. R. W. HUTCHINSON.

THE TEDA OF TIBESTI, BORKU AND KAWAR IN THE EASTERN SAHARA. *By* WALTER CLINE. No. 12, General Series in Anthropology. *Wisconsin*, 1950. 52 *pp.* (*one map*). *Price not stated.*

This new addition to an already well-known and useful series does not claim to be in any way original. It is a compilation from sources given in the Bibliography. After a short description of the country, the matter is grouped under the following headings :—the identity of the Teda and their neighbours (with paragraphs on the population, the clans, and the history of Tibesti) ; their economic, material and social life, and religious practices and burial. In his preface the author draws attention to the blankness of the best picture that can be drawn from his sources, and expresses the hope that 'some rugged young desert-lover will take my notes with him to Tibesti, learn the Teda languages, fill in all the gaping lacunae—and return to his university alive'. If he is an ethnologist there is much about the various clans of the Teda, their history and customs that he can add. The reviewer has no more first-hand knowledge of Tibesti than the author, although he does know their south-eastern neighbours and relatives the Zaghawa of northern Darfur ; and he is confident that there is much more to be found out, for example, about the past of the ruling section the Tomaghera that will add to the history not only of Tibesti but of Darfur. Incidentally the Birriarra do not ally themselves with *negro* chiefs of Darfur as stated on p. 44 but with their relatives the Tungur who are not of negro origin. And it is not correct to state as on p. 8 that no direct route from Tibesti to the upper Nile has been frequented in modern times. Fezzani traders regularly make the journey via Faya and Fada to El Fasher, and there is thence a much frequented road via El Obeid to the White Nile in the region of Kosti. There is no doubt much about the life of the Teda that our ethnologist can collect that will be of the greatest interest to pre-historians comparable to the impermeable basket for milk mentioned on pp. 29 and 39 which must have been a fore-runner of pottery in the history of domestic utensils. And if he is an archaeologist acquainted with the neolithic cultures of the Khartoum area and the Fayum, and able to make a sufficiently leisurely survey of the sites just mentioned on pp. 18–19, and the right choice of one of them for excavation on a subsequent expedition, he will probably find (for reasons that have already been stated on pp. 220–1 of Vol. XXX of *Sudan Notes and Records*), the key to the riddle why the Khartoum and Fayum cultures, although a thousand miles apart, have a remarkable number of features in common, one of which is the manufacture of amazon stone beads. There are only two localities where amazon stone has been found, and one of those is Tereneghei in NE. Tibesti. It is indeed not impossible that Tibesti holds the clue to the origin of the African strain in the civilization of ancient Egypt. Mr Cline's wish is heartily to be echoed—or, better still, may there, before long, be a joint Anglo-French expedition to this distant but promising area. Meanwhile we should be grateful to both the author and the editor of the series for drawing attention to a territory which is so little known and yet is probably of the greatest importance to the prehistory of North Africa. A. J. ARKELL.

SWEDISH ARCHAEOLOGICAL BIBLIOGRAPHY, 1939–1948. *Ed. by* SVERKER JANSON *and* OLOF VESSBERG. 360 *pages. Uppsala* 1951. 25 *Swedish Kroner.*

One of the commonplaces in archaeological research is the difficulty of tracing the papers of significance to any subject among the Proceedings of various societies. The Swedish Archaeological Society has not only produced an invaluable summary of works relating to Swedish archaeology for the war and immediate post-war years, but has also laid a challenge to other countries to publish a handbook and guide to the recent contributions to archaeological research in their own region.

The papers summarized or referred to in the bibliography total no less than 1244 and cover those written by Swedes and others on subjects of direct or indirect importance to Swedish archaeology as well as those by Swedish scholars on foreign spheres, classical archaeology, etc.

The book is in four main sections: Bibliography and History of Archaeology, Prehistoric Archaeology, Medieval Archaeology and Foreign countries, and these are subdivided under subjects and periods. Every aspect of research has been included, as is indicated, for example, by the subdivisions of the subsection ' Church Works of Art ' in the Medieval section: (1) Mural paintings, (2) Baptismal fonts and other stone work, (3) Triptychs, Icons and other wood carvings and paintings, (4) Stained glass paintings, (5) Tombstones, Mausoleums, etc., (6) Textiles, (7) Church fittings (communion silver, ciboria, reliquaries, thuribles, church bells, pilgrim badges, etc.), (8) Iconography.

As stated in the preface ' it is not claimed that the bibliography will be absolutely complete. Its authors themselves have chosen the works they have treated, and there are, therefore, certain variations in the selection and presentation of the different sections of the bibliography '. There is no doubt, however, that all the papers of any real significance are included. The different sections are written as a commentary on the problems of the subject in question and give the main arguments of the papers quoted. It is, of course, on the results of researches into the minutiae of archaeological remains that the broad outlines of prehistory and early history are based, and by their method of compiling the papers on detailed research the editors have produced not a mere reference list of books but a consecutive narrative that can indeed almost be read as an archaeological history of Sweden.

The absence of an index is perhaps to be regretted, but the bibliography is so arranged that it is comparatively simple to find the references to the subject required. The book is written in English and is, therefore, of great value to scholars without a knowledge of Swedish who would otherwise have great difficulty in finding comparative material in the Swedish literature.

It is intended to publish similar bibliographies possibly every five years, and if this hope is fulfilled the current archaeological literature of Sweden will become more easily accessible to scholars than that of any other area. The bibliography will be warmly welcomed by all students of Scandinavian archaeology and should find a place in all archaeological libraries. Perhaps we may also express the hope that a similar compilation of British research papers will follow as a result of the lead given by our Swedish colleagues. S.E.T.

SAXON ARCHITECTURE AND SCULPTURE IN HAMPSHIRE. *By* ARTHUR R. GREEN *and* PHYLLIS M. GREEN. *Warner and Son Ltd., Winchester* 1951. *Price* 12*s* 6*d*.

This is an admirable little book. The body of the book consists of a series of notes on the sixteen churches in Hampshire which have structural remains of Pre-conquest date. These are arranged alphabetically. There follow sections of notes on the sculptured Roods, i.e. representations of the Crucifixion, on the remains of crosses, on two sepulchral monuments, a grave slab and a memorial headstone and on the Saxon Sundials. A note on the well known miniature triangular ivory carving in the Winchester Museum is added for good measure.

The notes are well put together and summarize admirably the different published points of view of controversial examples. They are properly annotated and there is a bibliography. The plates are well reproduced and there are twenty of them. The text figures are adequate except for the plans which are extremely diagrammatic. No indication is given of the thickness of the walls which are shown by single lines solid or dotted. More substantially drawn plans would have greatly enhanced the value of the book. It is unfortunate also that no size is given of either the carved fragments in the Tudor House Museum at Southampton (though what seems to be a scale is just visible at the bottom of the photograph reproduced) or of the grave slab at Weyhill.

This is in short a book to be grateful for and it would be a very good thing if more such could be completed for other counties. The two new, or nearly new, examples published, the Southampton fragments and the Weyhill slab, especially the former, are considerable additions to our knowledge. The section on Sundials, a subject on which the Authors have long specialized, with its succinct introductory paragraphs is a particularly welcome feature of the book. G. WEBB.

ROMAN ANNIVERSARY ISSUES. *By* MICHAEL GRANT. *Cambridge University Press*, 1950, *pp*. XXIV, 204 *with two plates*. 21*s net*.

Professor Grant describes his book as ' an exploratory study of the numismatic and medallic commemoration of anniversary years, 49 B.C. to A.D. 375 ' and his purpose is to show that a large number of Roman imperial coins and medallions owe their types to coincidences in date with important anniversary years. Isolated instances of numismatic commemoration of anniversaries of notable events have already been identified by authoritative writers on Roman coins. They include the centenary issues of Vespasian of A.D. 69–71 and 73–74, recalling the victory of Actium and the ' restoration of the Republic ' in 27 B.C. Only six events have hitherto been associated with commemorative issues and the number of coins concerned has been few. Professor Grant extends the principle over the whole range of imperial coinage and attempts to show that some seventy different events and occasions were celebrated, often repeatedly on successive anniversaries, so that hundreds or even thousands of coins and many medallions would be brought into relationship. On this view the dates of particular issues would be deliberately chosen for their anniversary significance, or, in some cases, coincidences might be utilised for propaganda purposes. Several events therefore might be alluded to in a single issue. Thus it is claimed that 'the isolated issue of Claudius at Lugdunum recalls the fact that his own birth, 50 years previously, had occurred in the year of the dedication of the Ara Romae et Augusti (10 B.C.)—an event which was also to be celebrated by Carausius on its tercentenary. As well as commemorating the Lugdunum Altar, Claudius commemorates the half-centenary of the altars of Pax, Concordia, Salus and Janus (10 B.C. which had themselves been dedicated on an anniversary occasion'.

Professor Grant cites many instances of issues in which he finds, in the repetition of types, a composite anniversary reference to events of varying degrees of remoteness, 'Gordian III, in celebrating the three-quarter-millenary of the Republic, follows the precedent of Claudius who had inaugurated the same type (Libertas) on the imperial coinage just two centuries earlier. Then Carausius repeats the ROM.ET AUG. legend, which had last appeared 250 years earlier under Claudius; he also represents CONSTANT(IAE) AUG. just a quarter of a millennium after the inauguration of the type Constantia by that Gallic born emperor'.

This piling of anniversary upon anniversary with what Professor Grant calls 'a high degree of numismatic self-consciousness' may appear almost fantastic, but in support of his thesis he calls attention to the meticulous care with which the anniversaries of temple foundations were celebrated, doubtless from the records of the priestly colleges. This aspect of Roman religious thinking has already been stressed by Altheim, and anniversaries of temple foundations would relate to a considerable number of the coins concerned. Moreover the mint at Rome, which was in the temple of Juno Moneta, has a religious character, which extends to coin types and portraits. 'Moneta itself begins, before the death of Caesar, to appear on the coinage (on an anniversary occasion) as a religious concept, and before the end of the next century it has become Moneta Augusti'.

Professor Grant has an exhaustive knowledge of the Roman imperial coinage and supports his interpretation with a formidable body of evidence. Even if in some cases his conclusions appear to go farther than the facts warrant his book presents an important study of an aspect of the Roman coinage that deserves further attention. G.F F.

NEW LIGHT ON THE MOST ANCIENT EAST. *By* V. GORDON CHILDE. *Routledge and Kegan Paul*, 1952. *Price*, *£*1 15*s* od.

This is the third incarnation of a now famous book. The first was in 1928; and in re-reading H. R. Hall's review of it (ANTIQUITY, IV, 247–9) one is impressed by two things—the soundness of his judgment and the expansion of our knowledge during the interval, less than two decades in effect, that has passed since then. The second incarnation in 1934 (ANTIQUITY, IX, 236–7) was, as the author stated, no mere mechanical revision but a new casting; and this, the third, is the same. These recastings, foreseen by the author as well as by his reviewers, are an inherent necessity during a period when the framework—the chronological succession of cultures—is still being constructed. The additions made in recent years are due as much to an improvement in the technique of oriental excavations as to an increase in their number. Uruk and Hassuna have supplemented the deficiencies of Susa and Kish.

The book is concerned with one of the two most important and crucial problems of prehistoric archaeology—the transition from the hunting and collecting stage to that of food production, which led to the development of civilization. It was Professor Childe himself who, in another book (*Man Makes Himself*, 1936) first made us see the immense significance of the revolution brought about by agriculture; and in the book before us he describes the successive stages of that revolution. When he first outlined the process the earlier ones were still undocumented; in Mesopotamia, as he says, 'the neolithic prelude, previously only a postulate, has begun to take shape since 1941'. Now at Hassuna and Jarmo* we can begin to see the hunters abandoning nomadism for the more permanent kinds of settlement that food-production made possible; while the all-important last link, connecting the new regime with the old one, may soon be found in the caves of the northern uplands. Many years ago we suggested that it was here rather than on the plains that that link would be found, for if the last nomads (who were also the first food-growers) lived in caves they would presumably continue to do so for an appreciable time afterwards. 'Dr. C. S. Coon's excavations in a cave south of the Caspian have yielded a sequence beginning with a mesolithic and continued by a neolithic that has not yet connected on to any historically anchored culture-sequence' (p. 204).

It will be seen that the evidence still supports the priority of Mesopotamian over Egyptian civilization. Jarmo belongs to a pre-pottery stage and the 5th millennium B.C., but it is on the northern foothills; and in the middle of this millennium, if the chronology here tentatively suggested prove acceptable, the first temple at Ubaid in the south was built. At this date the inhabitants of the Egyptian Fayum were still neolithic barbarians. The priority is unlikely to be upset by future discoveries, if only because food-production is likely to have been invented in the region where food-plants grow wild, namely, in the north of the Fertile Crescent. At present one of the most obvious needs is for more excavation of neolithic and mesolithic sites in Africa, and for the radiocarbon dating of more of those already known. The date of the earliest of these sites in the Sahara and the Sudan is still unknown and subject to a margin of error of several millennia.

The format is excellent and a great improvement on the wasteful obesity of the 1934 edition. This 1952 edition, though longer—12 chapters against 10—occupies only 255 pages against the 326 of 1934. It is good to have the footnotes in their proper place again, at the foot of the page and not (as in 1934) at the end of the book. The illustrations have been reshuffled and some new ones inserted. The map, criticized in 1934, has been replaced by two others, which, though an improvement, are rather over-reduced and might be improved. We should like to see more maps that are as aesthetically pleasing as Miss de Cardi's in *Iraq*, XIII, 75. O.G.S.C.

CAVE EXPLORATIONS IN IRAN, 1949. *By* CARLETON S. COON. 10¾ by 8¼. *pp.* 124. 33 *plates, map, plans, sections. University Museum, Pennsylvania.* $1.50.

This monograph contains the report of a journey in search of caves undertaken from June to October 1949 in the limestone country bordering the Iranian plateau—country which from the point of view of prehistory is virtually unexplored. Dr and Mrs Coon excavated four sites, all in Iranian territory; Bisitun, 30 miles east of Kermanshah, Tamtama on the west side of Lake Urmia, the Ghar-i-Khamarband, or Belt Cave, on the southern shore of the Caspian Sea, and a rock-shelter at Khunik on the eastern border of the plateau 100 miles from the Afghan frontier. Only in the western part of this area had any previous Palaeolithic finds been made, when in 1928 a joint expedition of the Sladen Fund and the American School of Prehistoric Research dug two caves in the Sulaimani region, Zarzi and Hazar Merd, which yielded Gravettian and Mousterian industries respectively. Of the four caves investigated by the Coons, Tamtama contained a Pleistocene fauna and a fragment of human femur, but a handful only of atypical stone tools, while, at Khunik, Mousterian artifacts were abundant, but lay on the surface of an early Islamic village dump, having apparently slipped down from a collapsed cave higher up the cliff. Bisitun and the Belt Cave, however, yielded interesting stratified material.

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* See ANTIQUITY XXIV, 1950, 190–6.

At Bisitun, which is 4,500 ft. above sea-level, a south-facing cave contained a Mousterian industry closely comparable with that of Hazar Merd, which lies at approximately the same height, 140 miles away to the N.W. There are similar elongated triangular points, often very narrow, about 50 per cent having the striking-platform retouched, and as at Hazar Merd, truly typical racloirs are comparatively infrequent. In both sites the untrimmed material included a high proportion of narrow flakes, some of which can fairly be described as blades. There were no cores at Hazar Merd, and those from Bisitun are not described, and were apparently in large part rejected after counting, so a study of the striking-technique of this industry has yet to be made. It is clear however that many of the points are made on triangular Levallois flakes, and the narrow forms most probably come from double-ended cores. Animal bones were more abundant at Bisitun than at Hazar Merd, but do not add greatly to the list of fauna. The leading forms at all levels are red deer and horse. Coon notes a very high proportion of mere splinters of long bones as compared with those carrying articular surfaces, and puts forward the theory that the cave was a sacred store-house in which splinters and teeth were placed as offerings to represent the game killed, the joints apparently being gnawed on the talus (which was not excavated). This explanation is more ingenious than convincing.

The Belt Cave revealed a very interesting sequence from Mesolithic to Neolithic, the former being of a type which does not tie up with anything so far known in a radius of 500 miles, although it is possible that if the material from the Caucasian caves were available it might throw some light. The flint industry is characterised by rather rough triangles and trapezes, both tending to have the retouched edges concave ; two of those figured might in fact be described as hollow-based arrowheads. Others have a dorsal projection which suggests that they were hafted transversely (a practice which may be related to the considerable number of bird bones found in the cave). Associated with these geometric forms, which are most abundant in the lower levels, are curved blunted-back knives, which increase in the Upper Mesolithic, finely-pointed borers, and end-scrapers. There are no micro-burins, and none of the implements are very small. Bone tools include three rubbers or chisels of deer antler, and some polished antler tips whose use is not obvious. Ornaments consist of pierced land and sea shells, pierced teeth of seal and ass, and two polished stone pendants. Two mortars, with traces of ochre, seem to have utilised naturally shaped stones, and the pestles also look like unshaped pebbles.

The upper layers of the Belt Cave yielded flint sickle-blades and knives, polished stone celts, and a variety of sherds of most periods up to the Islamic ; 174 fragments of poorly baked, soft-centred pottery are however considered to be probably Neolithic. A Carbon 14 analysis of burnt bones from the zone in which this ware alone is found gives a date of about 6135 B.C., but as the C 14 dating for the Upper Mesolithic of the Belt is 8160 B.C., while for the Lower Mesolithic it is 6054 B.C., it is clear that something has gone seriously wrong, and that for this site at least the method cannot serve as a guide.

In the Mesolithic fauna the gazelle predominated, although *Bos primigenius* was also present, and wild sheep and goat began to appear in the later stages. A large and heavy form of *Canis familiaris* occurs at an early stage, recalling that in the Natufian also the only domesticated animal is the dog. The Belt Cave is four miles from the Caspian Sea, and seals (a new species, not the *Phoca Caspica* of to-day) were hunted throughout its occupation, but more especially in the Lower Mesolithic, the proportion of immature animals suggesting that the seals with their cubs were clubbed at their breeding-places.

The Mesolithic of the Belt Cave has nothing in common with the Natufian, which has not so far been found outside Palestine, Syria and Lower Egypt. Nor does it resemble the last stages of Zarzi, geographically much nearer, where the Gravettian gives rise to very small crescentic microliths, button-scrapers and micro-burins. The industry of the Belt may prove to be a specialised culture of the Caspian shore, the product of a mode of life different from that of the mountain dwellers of Kurdistan.

Dr Coon's results are clearly presented, but would be even easier to use if the results from each cave were grouped together, instead of being spread throughout the book. Some of the analytical tables are rather heavy going, but in general they seem to be justified by results, since most of them bring out facts which could not be so convincingly presented by a mere description. We await with interest the results of his second expedition in this region, undertaken in 1950–51. D.A.E.G.

STIERNHOLM LEN, 1603–61. *By* HANS H. FUSSING. *Copenhagen, Einar Munksgaard,* 1951. 312 *pp. Price* 35 *Danish Kroner.*

This book contains an analysis and discussion of the annual accounts submitted to the Exchequer by the officers successively appointed by the Crown to take charge of one of the large administrative districts into which 17th century Denmark was divided for fiscal purposes. It is a highly technical study, written in Danish, of an economic system which cannot easily be understood by those unacquainted with the ground. Fortunately for the English reader, an appendix of eight pages gives an English summary of the main points brought out by the accounts. To the student of Danish economic history, for whom the work is intended, this carefully tested material will obviously be of high value. On the other hand, it may disappoint those who approach a book of this kind in the hope of finding an occasional parallel to the social and tenurial peculiarities of the English Danelaw. The system which it describes is an artificial organisation overlying, and to a large extent obliterating the primitive agrarian order of Denmark. Even so, the exaction of rents in kind and labour services from peasants who are clearly independent in their social relationships is worth noting by those who would understand the condition of the medieval sokemen of eastern England. With the possible bearing of his results on problems of English social history the author of this study has, naturally, no concern. What he has produced is an excellent piece of objective research, which will provide a firm basis for future investigation. FRANK M. STENTON.

DANSKE OLDSAGER, II, YNGRE STENALDER. *By* P. V. GLOB. 139 *pp.* + 72 *plates. København* (*Gyldendalske Boghandel*), 1952.

In all Europe, Denmark is the land richest in prehistoric antiquities so that Danish types are well fitted to provide a standard series for other regions. And, thanks to the devotion of Danish prehistorians, they are the best published too. Already in 1836 Thomsen's famous *Ledetraad* established the Three Age system to be followed six years later by Worsaae's work, best known here in the English translation, *The Primeval Antiquities of Denmark* (1849). This was followed in 1888 by the first volume of Sophus Müller's *Ordning af Danmarks Oldsager*—an invaluable atlas illustrating all the principal types of Stone Age relics then recognized with a short descriptive text in Danish and a French résumé. Since that date the number of types has enormously increased both by surprising new discoveries and by refinements of classification so that a new edition has become indispensable. The discovery of the Maglemose culture, unsuspected in 1888, and subsequent finds has made it necessary to devote a whole volume to the first part of the Stone Age, the *œldre Stenalder* of Sophus Müller's atlas, which was edited by T. Mathiassen and published in 1950. To this Glob's work forms the sequel. He illustrates and describes 637 types of the New Stone Age as against the 259 which had sufficed to characterize both the mesolithic and the neolithic in 1888. These are further divided between three main periods—Early, Middle and Late Neolithic—while those of the second period are further distributed between the Passage Grave, Pitted-ware, Battle-axe, Bell-beaker and Boat-axe cultures. Otherwise Sophus Müller's plan is followed save that line-drawings are largely replaced by half-tones and the résumé is in (excellent) English and very much fuller (58 pp. corresponding to 73 pp. of the Danish text !). Since metal was not unknown in ' Neolithic ' Denmark, Prof. Glob has very properly included the copper disk from an Early Neolithic grave at Salten and the Middle Neolithic Bygholm hoard, together with a stray halberd and lunula, and figured a number of bronze daggers, celts and pins together with the Late Neolithic objects of stone and bone which often imitate them. Seventeen ' objects from various periods ' figured at the end include a genuine trough quern traceable as early as the Middle Neolithic and an alder dug-out canoe of like antiquity. The English description does not include the site or context of each specimen but this can easily be read off in the corresponding Danish text. Many of the objects illustrated are not otherwise accessible and English readers will be surprised to find something familiar, for instance in No. 433—a four-footed food-vessel ! V. GORDON CHILDE.

MONASTIC SITES FROM THE AIR. *By* DAVID KNOWLES *and* J. K. ST. JOSEPH. *Cambridge University Press*, 1952. *£2 15s od.*

This splendid picture-book contains 128 air-photographs, taken by Dr St. Joseph, of monastic buildings or sites in England, Wales and Scotland. They are grouped in the Orders to which they belong, and the text follows the plates, as it should ; that is to say, each plate has its accompanying description with or opposite it. These descriptions are terse and in every way admirable ; they give just so much information as the reader requires, including always the relevant dates and some account of the topography. The bibliographical information does not pretend to be complete but seems quite adequate. The authors have produced a book which is both easy to use and pleasant to look at. The only omissions that one might criticize are of the points of the compass (though these are given in the text) and map-references, but here again the text always gives enough to locate the position, and there is an index-map of sites at the end of the book. Moreover the recently published Ordnance Survey map of Monastic Britain, which all serious students will possess, marks all these (and many more) remains, and will be found a useful adjunct to this book.

There is no other book which covers the same ground, and it is fortunate that the descriptions should be by Professor Knowles whose great learning and authority in monastic studies is happily combined with a pleasant and attractive style. It would have been easy for one less urbane than he to fall foul of the orgy of vandalism released by the Reformation, of which the illustrations are evidence. One does not need to be a Catholic to be stirred to anger by the wanton destruction of so much that was fine and beautiful, and to one who is, that destruction must be even more painful. There are, however, in this book instances of the re-occupation by monastic Orders of some of the earlier sites that had become ruined. These are as yet fewer in number than lay occupations, but they are, in the opinion of many, equally regrettable. The case of Fountains Abbey is mentioned where public opinion was roused and the scheme dropped. One can readily understand the motives which lie behind the policy of re-occupation ; but at this late date, after a break in building traditions of some four centuries, the old buildings should surely be allowed to rest in peace, and a new foundation, if it must be created, should be (as at Quarr) adjacent to them, or elsewhere altogether.

These oblique air-views are probably the best pictures that could be taken of the ruins, and it is no detraction from their excellence to say that, good though they are, they do not as a rule add much to what was already known. Only four in this book are claimed as doing this. One of these is Sempringham ; and the air-view of the site had, for the present writer, another interest also, for it showed how complete has been the obliteration. Those of us who are not specialists are nevertheless familiar, from our readings, with the more famous monastic establishments, even if we know little more than the names ; and it is very interesting to see from their pictures how much or how little now remains.

The book is described as ' the first of a Series of books to be known as *Cambridge Air Surveys*', edited by the authors of this book and Professor Godwin. No doubt there were good reasons for selecting monastic sites for the first of the Series; but those who know what vast riches are crowded together in a small space—far *too* small a space—in the Cambridge air-photographic archives will await with eager expectation the appearance of the next volumes. We do not know what these will be, but a volume on Roman sites and another on prehistoric ones are obviously indicated. The as yet almost unpublished wealth of the Fenland in Roman-British field systems will one day, we hope, be adequately recorded in the series ; but they require verticals rather than obliques. Cambridge has now become one of the leading archaeological centres of Britain. The official status given to archaeological air-photography was a wise and far-seeing act ; for the subject is one that requires access to a large library and map-collection. Conversely, a Faculty of Archaeology nowadays is incomplete without a collection of air-photographs to draw upon for teaching purposes (lantern slides especially). Its students, who will some day occupy leading positions, must have access to such a collection or lose a vital part of their training. The inauguration of this new series by the Cambridge University Press is proof enough that Dr St. Joseph's little Department has already amply justified its existence.

O.G.S.C.

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